

Psychological Bulletin

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Psychological Bulletin

SOUND-PRECIPITATED CONVULSIONS: 1947 TO 1954¹

WILLIAM BEVAN
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Sound-induced convulsions are still a scientific enigma. In the more than six years since Finger's (27) excellent review, nothing has been constructed that approximates a satisfactory explanatory framework. This does not mean that the phenomenon has lost its challenge. In the period covered by the present paper, approximately 129 audiogenic seizure titles were available to this reviewer. A check through Finger's bibliography revealed 106 titles, going back to the earliest reports of the syndrome. But there have been shifts in interest. The grinding of theoretical axes has more or less ceased and attention has turned toward cataloging—somewhat unsystematically—the variables that influence seizure susceptibility. Less of the work is being done by psychologists. Of the 106 papers cited by Finger, 92 were by psychologists, 14 by nonpsychologists; 90 appeared in psychological publications, 16 did not. In contrast, of the 129 items appearing in the past six years, 66 were authored by psychologists, 63 by nonpsychologists; 59 appeared in psychological publications, 70 were in nonpsychological journals. All of Finger's references were to American or Canadian journals; of those added

after 1947, 19 have appeared in European publications. Recent work varies greatly in quality: some studies involve insufficient numbers of animals, inadequate design, and/or inept handling of behavioral data. Other experiments are exceedingly well done. Perhaps the most promising developments are concerned with the inheritance of susceptibility in mice, stimulus priming, and learned control of the seizure. The predominant theoretical bias is still physiological, but there is increasingly clear recognition of the overly simplified nature of earlier interpretations and of a need for a view flexible enough to accommodate the complex of data recently acquired.

The present review was planned as a supplement to Finger's. Thus it has been given approximately the same form.

PROCEDURES FOR TESTING AND FOR ANALYZING TEST BEHAVIOR

There has been little change in testing procedure since the early years of audiogenic seizure research. Experimenters most frequently use a small wash tub or other container of similar size as a test chamber. The most widely used sound-source is a door bell generating 90- to 100-db noise. Some workers, for a variety of special reasons, use pure-tone stimuli

¹ The writer is indebted to J. L. Fuller of the Roscoe B. Jackson Memorial Laboratory, Bar Harbor, Maine, for reading critically the first draft of the present manuscript.

produced by a variable-frequency oscillator or compressor-driven Galton whistle. Few, if any, continue to use the air blast or jingling keys. The duration of the sound stimulus seldom exceeds two minutes, since the probability of a response after this time is small (124). Animals are usually allowed free movement within the confines of the test chamber, since complete restraint has generally been found to reduce sensitivity. Since too frequent testing has a similar effect, tests in a series are usually spaced at two- or three-day intervals. Some experimenters, however, have tested as frequently as twice daily (18, 37). A few workers (37, 54) include observations of the subjects' (Ss') behavior for a short period prior to presentation of the auditory stimulus.

Despite criticism (101) that the stimulus lacks sufficiently precise definition, little concern has been directed toward further specifying the physical properties of the convulsion-provoking sound or clarifying the significance of the nonauditory characteristics of the test situation. Only fairly recently has a formal attempt at standardization of test conditions been made (36, 124). Because of their interest in audiogenic seizures as a technique of physiological and genetic assay, the division of Behavior Studies of the Jackson Memorial Laboratory at Bar Harbor, Maine, has undertaken to serve as a clearing house for information on seizures. This group recommends a two-minute test in the bell-wash tub apparatus with 30-day old mice, bred for sensitivity or immunity to seizure.

Most workers divide the observational sequence into two or more phases:

Prestimulus behavior (recorded by a few investigators). This involves a description of the variety of actions

displayed by the S in its home cage and in the test chamber just prior to being exposed to sound. Crude estimates of amount and vigor of activity are frequently included.

Preconvulsion behavior. The S's behavior from the onset of auditory stimulation until the appearance of the violent behavior that characterizes the fit. Data from this period include descriptions of premonitory (i.e., the crouching, shivering, burrowing, etc., that precedes convulsion) and substitute behavior (i.e., the preening, washing, exploring, and attacking the sound source that may appear on nonconvulsion trials). Occasionally estimates of the frequency and duration of the several symptoms are included.

Convulsive behavior. That phase of the S's reaction characterized as wild, violent, uncontrolled, or quasi-uncontrolled. Typically, concern is with the number, and sometimes the duration, of convulsive reactions in a group of animal Ss or in a single S on a series of trials. Reactions are classified as running, subconvulsive, abortive, or disturbed when the animal's reaction does not progress beyond vigorous, apparently blind, running and circling movements (7, 88, 101). Full convulsive attacks involve spasms and clonic and/or tonic-clonic episodes. An additional category may be used if the convulsive reaction is so severe as to compel interruption of the test to prevent death or if, as may occur in a sequence of tests, a modified "controlled" pattern appears (55, 84).

The most common type of quantification consists of the relative frequency of convulsion, or various types of convulsion, in a group or in individual Ss for a series of tests (7). Such scores may be submitted to probit analysis (31). Other forms of scoring have been more recently in-

roduced. Estimates of severity have been derived by assuming that the ratings from "no reactions" through "running" to "tonic-clonic attack" form a linear progression and thus may be summed or averaged (8, 88, 89). Latency measures and duration measures have also been obtained for the running attacks and the tonic-clonic episode (7, 8, 33, 45, 55, 96). The mean number of running attacks as well as the relative duration of the pre-epileptoid running have also been useful indices (8). No one, however, has yet attempted to devise a chronaxie-like score that incorporates both intensity and duration.

Postconvulsive behavior includes coma and *cereae flexibilitas* or occasionally "epileptoid furor." This infrequent "bouncing response" is usually associated with less severe tonic-clonic attacks. Reports may also include *recovery time* (the duration of the interval between the cessation of convulsive movements and the appearance of "normal" movements [101]) or *death time* (the interval from the onset of the stimulus until relaxation of the flattened pinnae [44]). Occasionally an appraisal of the S's sensitivity to various forms of further stimulation is made (101, 128).

VARIABLES INFLUENCING SEIZURE SUSCEPTIBILITY

Characteristics of the Seizure Situation

The rigidity of early views concerning the relevance of certain aspects of the test situation for seizure-induction has precluded extensive empirical delineation of their significance. Though a few investigators (28, 96, 103) have emphasized that sound as both a sufficient and necessary condition has not yet been adequately established, the majority of investigations to date have been conducted on this assumption.

"Crucial" Sensory Modality

Audition. A few studies have dealt with the relative effectiveness of different kinds of auditory stimulation in provoking attacks. Comparisons of simple and complex sounds generally indicate the superiority of noise (bells, jingling keys, etc.) over pure tones (80, 104). This may be due to nonsonic stimulation (vibration of the test chamber) or to the acoustic property of complexity (31). When the noise of jingling keys was transmitted through an acoustic system which filtered out frequencies above 6,000 cps, seizures failed to occur; in contrast an effective band was found between 8,000 and 11,000 cps, the upper limit reproduced (104). An arrangement of three oscillators, with the high frequency beating against the low, was as effective as keys: the optimum high frequency, 9,500 cps; the low between 50 and 500 cps modulated two or three times per second; the minimum intensity, 20 baryes. Pure-tone experiments indicate the most effective frequency to lie above 8,000 cps. Morin, *et al.* (104), working with rats, suggests 9,500 cps as the optimum; McGrael (80) using DBA mice, a peak range between 10 and 12 kc. Frings and Frings (32), also using mice, report the range of maximum effectiveness to be between 12 and 25 kc. with seizures evokable by frequencies as high as 35 kc. Most investigators have used stimulus intensities of about 100 db. Frings and Frings (32) note that with a 10 kc. stimulus, a decrease in intensity from 110 to 95 db results in fewer seizures and longer latencies. Furthermore, greater intensity (median 105 db) is required to initiate than to sustain seizures (95 db) and less to initiate convulsions in subjects with a history of seizure (83 db) than in those convulsing for the first time (92 db) (31).

The Morin data on synthetic noise suggest that its superiority may lie in its aperiodicity (104). An intermittent pure tone (9,500 cps, 20 baryes) was found to be most effective with three-per-second interruptions, a rate similar to that of certain bioelectric potentials in epilepsy. Prerequisite duration of sound stimulation is also of importance. During a single test, this is most obviously correlated with frequency and intensity, some combinations of these dimensions being more effective than others. But the induction process is probably not a simple monotonic shift toward threshold. Investigators have long known of an upper time limit for the occurrence of the reaction. Frings and Frings (31) note that few seizures occur in their mice after one minute. Using the classical priming method of physiology, Fuller and his collaborators (43, 44, 45) have shown number of animals convulsing and the mean convulsion latency to vary with duration of the conditioning stimulus and of the conditioning-test stimulation interval. Stimulus priming of 5 to 10 seconds plus an interval of silence up to 60 seconds resulted in a facilitation of seizures. Fifteen seconds of priming plus a 40-second interval produced a reduction in seizure risk. Twenty seconds of priming and 40-60-second intervals yielded better protection. Intervals of 80-100 seconds were associated with a return to normal susceptibility.

Number of previous tests and/or their temporal distribution are other relevant variables. It is common practice to test rats on alternate days in order to allow sufficient time for recovery. Maier, Feldman, and Longhurst (84) report animals tested at weekly intervals to have a higher proportion of seizures than those tested daily over a comparable period. Meanwhile, as the number of

tests increases, the pattern of dominant response tends to shift, the diffuse reaction being replaced by a controlled forepaw clonus and post-convulsive hyperexcitement (18, 55, 84). The greatest number of maximum reactions tends to occur on the first tests (142). While latency of running has been variously reported to increase from the beginning to the end of a test series (96), and to be high on the first reaction but lowest in the middle of the sequence (37), number of successive seizures is regarded by some (32, 33) to be a function of the animals' physiological condition rather than the number of previous tests.

Other modalities. Neither of the early explanatory biases—conflict or sensory overflow—require the assumption that auditory stimulation is uniquely necessary to the occurrence of the fit. Thus several experimenters have explored the possible effects of intense excitation of a non-auditory sort.

Intense light. Shafer and Meyer (125) exposed rats to intense illumination as well as to combinations of light and sound. No convulsions, either running or tonic-clonic, were provoked by light alone; those obtained with combinations of light and bell occurred no more frequently than with bell alone. Goldberg (54) attempted to condition the fit to intense two-per-second flashes of light. In a preconditioning series, light alone elicited no seizure symptoms. In the nonreinforced test trials, several bursts of wild running were noted. The experimenter, however, is hesitant to regard these as conditioned convulsive reactions.

Pain. Several experimenters have commented on the possible significance of the extra-auditory pain pathway. Marx and Jurko (88) suggest that irritation and increased

tension on the tympanic membrane may reduce the pain threshold and thus facilitate seizures. Shafer and Meyer (125), pointing out that the pinna is supplied by the trigeminal nerve, propose that susceptibility may be associated with pinna irritability.

Electric shock has been described as an effective stimulus on several occasions. The present writer has observed a severe epileptoid reaction in each of two rats forced with intense shock at the choice point of a gridded Y maze. Meanwhile, Lazovik and Patton (75) report inability to induce convulsions in rats forced with shock to respond to an unsolvable problem arranged for the Lashley jumping stand, even though the subjects were sensitized by a magnesium deficiency. Nor was Coleman (21) able to condition the seizure to shock, although he could vary the pattern of reaction by changing the intensity of shock presented with an effective auditory stimulus.

Temperature. Early reports indicated that temperature affected susceptibility although the extent of its influence was confounded by concomitant exercise effects. In a recent experiment Fuller and Rappaport (42) found that mice immersed in 5° and 23° C. water immediately prior to test were rendered immune, and convulsive risk in those subjected to 38° C. water significantly reduced. Animals wet after the convulsion began, however, received no protection. The susceptibility of those dried before testing was similarly unaffected. Meanwhile, severity increases with increase in the temperature of the test chamber (125).

Proprioception. Speculation has frequently placed proprioception among the "crucial" nonauditory factors. Observed changes in sus-

ceptibility coincident with rotation of the animal, restricting its area of movement, binding it and subjecting it to forced exercise antedate the period of this review. Vigorous running and awkward backing and sidling movements are included in all descriptions of the audiogenic attack. Chance's description (20) of a strain of mice that convulse when gently shaken sharply contrasts with Fring's (37) report that mice may sometimes be revived by such treatment. Two recent studies have dealt with the possible effects of damage to proprioceptive structures upon seizure susceptibility. Finger, Bice, and Day (28) found injections of streptomycin induced impairment of righting and motor coordination not seen in control animals treated with dihydrostreptomycin or isotonic saline. No significant differences occurred in the proportion of convulsing rats per treatment group or in the total percentage of convulsions. This supports Marx and Jurko's (88) report that labyrinthitis is not a sufficient cause for convulsions. However, no appraisal was made of the extent of vestibular damage and the criteria for evaluating possible behavioral effects were rather crude.

Bevan and Hunt (8) obtained no significant differences in the frequency or severity of attacks, nor in the latency for the first running attack, in magnesium-deficient rats tested before and after bilateral cervical dorsal hemisections. However, the operated animals had significantly longer latencies for the epileptoid phase, spent a longer total time and a greater proportion of the interval prior to tonic-clonic attack in running. The tonic-clonic episode, furthermore, appeared to be shorter and less severe in the experimental group. Meanwhile, it is not clear

that the experimental lesions were sufficiently extensive or that the dorsal columns in the rat are proprioceptive in function.

Regulation of Free Movement

Interference with the S's movements is a demonstrated variable. Its effects have been variously attributed to shift in proprioceptive inflow, gradual rather than sudden motor outflow, and enhanced conflict. The techniques for effecting interference have varied with the speculative bias of the experimenter. Cain and Naquet (15) have corroborated earlier reports that complete immobility through binding affords almost complete seizure protection. Confinement in a small cage or glass cylinder yielded the same results. Cain, *et al.* (14) claim similar results when subjects were bound for EEG recording. Griffiths (61) reports that rats placed in a Carmichael sock to facilitate the attachment of EEG and EKG electrodes, and tested either in the sock or in a larger box, showed almost no responses. In contrast, a high incidence occurred in rats tamed to the point of permissively allowing the electrodes to be attached; speculation about tameness and neurosis may thus be relevant. The restrained ungentled rats engaged in violent clawing, shaking, biting, and scratching at the electrode leads, the tamed unrestrained showing almost no manipulative behavior at all. Frings and Frings (31) report failure to provoke attacks in mice suspended in the test chamber by means of a spring paper-clip, while Rabe (121) claims shorter latency, longer convulsions, and quicker death in subjects confined to a small cage inside the tub. Chance (16) similarly describes greater responsiveness in amphetamine-treated mice tested in small

jars as compared to larger boxes. Michels and Bevan (96) meanwhile arranged a situation for altering the size of the test cage without changing the acoustic properties of the auditory stimulus and found no variation in frequency of either running or full convulsions coincident with shift in cage size. Latencies for the first running attacks, however, were longest in the smallest cage. This suggests that under certain circumstances, sensitivity may be directly rather than inversely related to activity level. In line with this are reports of 18 of 29 rats having seizures when tested in a rotating drum (15).

Opportunity for Escape

This may be described as a set of conditions that allows the animal S to manipulate or otherwise act upon some aspect of the test situation with the result that a convulsion is averted. Conflict-oriented experimenters have interpreted the confinement and restraint situations as ones in which the animal is driven to respond when response is impossible. Griffiths' data, just previously cited, suggest that directed activity is prerequisite to immunity. Maier (85) attributes the general ineffectiveness of electric shock in the forced jumping situation to its being a localized stimulus capable of eliciting a specific response and Marx (89) suggests that "substitute behavior" is effective because of its pain-reducing influence. If an aspect of the situation draws the S's attention away from the noxious stimulus, enabling him to "go out of the field," it may provide seizure protection. Marx and Van Spankeren (89) trained animals to cut off the auditory stimulus by tilting a pole and observed fewer running attacks and tonic-clonic episodes than in an untrained group even with delays in the presentation of the pole up to 64 sec-

onds. Further evidence of control is the predominance of forepaw attack in the trained animals. Goodson and Marx (55) confirmed these results with an improved design involving a wheel-turning response. A comparable incidence of reactions occurred in E and C groups when the experimentals were tested without access to the wheel. More recently Chance (18) has arranged to introduce intermittently a plasticine hut into the test situation as a "refuge" for the S. On the first and second series of tests, introduction of the hut was accompanied by a suppression of the convulsive response persisting for several days even though the hut was removed shortly after the sound began. In a third series a horseshoe of the same dimensions was substituted for the hut without affecting the results. Typically, when the hut was not present, animals displayed searching movements, creeping to the place where the hut had been and relaxing.

Characteristics of the Seizure-Sensitive Animal

The convulsive syndrome is dictated not only by the external conditions that comprise the test situation, but by states of affairs within the animal S. These latter may be constitutional factors, specified in physiological or psychological terms, or experimentally produced pretest changes in physiological or psychological status. Statements about individual differences in susceptibility are simply recognitions of this fact. Rat colonies containing inbred stock yield up to about 50 per cent susceptibles. Mouse strains have been developed in which both the convulsive and death risk is high, in which convulsive risk is high but death risk low, and in which both convulsive and death risks are low—

but in no case are risks either zero or 100 per cent.

Genetic Differences

Experimenters have generally agreed that such differences are important. Since in most cases, however, it has been impossible to specify the purity of the test stock, or assess possible confounding by differences in nutritional status, age at test, and experimental treatment, the relevant rat data defy confident interpretation. Even with homogeneous mouse stock such considerations as age at test (37, 132) and criteria used to express responsiveness (40) have proved of critical importance.

Patterns of reaction. In a 1951 study Frings *et al.* (37) reported no strain differences in convulsive pattern among mice used. Vicari (134) in the previous year had reported three patterns that varied with age and strain: violent running plus tonic-clonic convulsions and death; less violent running and convulsing with few fatalities; and a static and paralytic reaction with no deaths. In a later study, Frings and Frings (32) describe DBA/1 mice as highly susceptible to fatal tonic-clonic reactions, while the responding C57/6's display a long tonic period. There are also differences among the specially developed Frings strains (30): one shows frequent clonic, few tonic-clonic reactions; another a high tonic-clonic rate, but few fatalities. The postepileptic phase in rats has been generally one of coma and reduced activity. Chance (17), in contrast, describes a terminal saltatory attack in *Peromyscus* during which the subject bounces with retroverted head, attacking other mice, or models, and sometimes biting the floor. The present writer has observed the same pattern in young Sprague-Dawley rats sensitized by a magnesium-

deficient diet, although no tests of attack behavior were made.

Frequency of convulsive reaction.

The first experiment comparing mouse strains on responsiveness was conducted by Hall (64), who found that 93 per cent of a group of Bar Harbor DBA animals gave violent tonic-clonic reactions, while only 11 per cent of his C57 sample convulsed. Three DBA transplants into C57 mothers all convulsed. About the same time Vicari (130) reported similar differences for these strains, and in addition 83 per cent susceptibility in the CE (extremely dilute) strain after 55 days of age, and 28.5 per cent in Albino A at 5 months. Lindsey obtained 80 per cent convulsions in 34-day old DBA's on a single test, 21 per cent in Albino A, 9 per cent in Albino C, 2 per cent in C3H, and none in C57 (77). Witt and Hall (142) crossed DBA/1 and C57/6 and got 90.5 per cent reactors in the F_1 and 77.3 per cent in the F_2 . Backcrossing F_1 with C57 yielded 52.8 per cent, with DBA 90.9 per cent. Finally, a critical backcross of C57 with nonreactors of the C57 backcross gave 25 per cent. Fuller (39) crossing DBA/2 with C57 obtained 32 per cent and 58 per cent in the F_1 and F_2 respectively, 92 per cent in DBA B_x and 23 per cent in the C57 B_x . Ginsburg, Miller and, Zamis (50) in crosses of DBA/2 and C57/10 got 40 per cent more seizures in the F_1 and F_2 than when DBA/1 was bred to C57/10. Replacing Witt and Hall's four tests with five, and reactor-nonreactor classification with scores of convulsive risk, Fuller, Easler, and Smith (40) obtained F_1 's with 68 per cent and 79 per cent convulsers, F_2 , with 70 per cent, and $F_1 \times$ DBA and $F_1 \times$ C57 with 95 per cent respectively—all lower, with the exception of $F_1 \times$ DBA, than the proportions of Witt and Hall. Later

Fuller and Williams (45) describe a convulsive risk of 11 per cent for the A/He strain and the following for hybrids: DBA/2 \times C57, 76 per cent; DBA/2 \times A/He, 91 per cent A/He \times C57, 4.5 per cent. In a comparison of DBA lines 1 and 2 and C57 lines 6 and 10, Miller, *et al.* (98) report an 81 per cent incidence for DBA/1, 98 per cent for DBA/2, while both C57's were less than 5 per cent. In F_1 , however, hybrids of C57/10 showed a greater incidence than those of C57/6. All F_2 's were lower than F_1 with the exception of DBA/1 \times C57/6 which remained about the same.

Severity of reaction. Little has been done with severity as a scalable parameter. Witt and Hall (42) classed reactions into three intensity categories: running; running plus tonic-clonic episodes; running, tonic-clonic attack, and death. If these categories be assumed to represent equally spaced intervals on a three-point scale, a composite severity score may be derived by summing the products of these weights and the relative frequency scores for each category. Witt and Hall's groups rank as follows on this criterion: DBA, very severe; F_1 and DBA B_x , slightly less severe; F_2 severe; C57 B_x , moderately severe; C57 and C57 B_x , nonreactors of C57 B_x , of negligible severity.

Latency of reaction. Strains differ in their readiness to succumb to the seizure-inducing stimulus. Fuller, *et al.* (40) present significantly shorter mean convolution latencies for DBA and $F_1 \times$ DBA (35.8 sec. and 38.9 sec.) than for F_1 and $F_1 \times$ C57 (55.2 sec. and 54.5 sec.). Latencies in the last two groups, and possibly F_2 , were bimodally distributed. Bimodality in DBA/2 is more clearly demonstrated in a later study (44). A/He is considerably slower to react than

DBA, having a mean latency of 61.8 sec. (45); hybrids of A may be slow or fast, depending on the nature of the cross; $A \times DBA$, 35.6 sec.; $A \times C57$, 54.6 sec. In contrast, Frings, Frings, and Kivert's data (37) suggest no difference in the latency for the first running attack for DBA, C57, and mongrel Swiss Albino (average times: 33 sec., 31 sec. and 33 sec.). A later paper (32) reports average latencies of 26.6 sec., 26.1 sec., and 21.0 sec. for these same strains.

Death risk. Unless suffering from severe nutritional defect, rats only rarely succumb. Not so with mice. Hall (64) reported 87 per cent fatalities in his DBA subjects, 7 per cent in his C57 group. Vicari (130) has obtained similar results: at 30 days of age 67 per cent of her DBA died; at 35 days fatalities were reduced to 44.4 per cent. In addition, while her CE's, Albino A's, and C_3H 's differed in responsiveness, all were free of casualties. A later investigation (135) reports the highest DBA mortality to occur in the period 30-39 days. Lindzey (77) notes the following strain differences in 34-day old samples: DBA, 75 per cent deaths; Albino A, 1 per cent; C57, C_3H , and Albino C, all zero. Frings' (37) mongrel Swiss Albinos show slightly greater inclination toward fatalities than C57. More recently, Frings and Frings (34, 35) have developed two strains that are highly responsive but show exceedingly few deaths. Thus susceptibility to seizure and capacity for recovery appear to depend on independent mechanisms. This is further indicated by Fuller, Easler, and Smith's (40) failure to discover a significant relationship between latency and fatality and their finding of the same convulsive risk in DBA and backcross of $C57 B1 \times DBA$ with lower death risk in DBA, as well as

by Fuller and Williams' report (45) of high convulsive risk but low death risk in the hybrids of A and DBA/2. It is highly probable that C57 would succumb readily if they were less seizure resistant.

Age of first reaction and greatest sensitivity. Mouse strains differ in both the mean age of first convulsion and period of greatest sensitivity. While Vicari reports 78 per cent reactors in DBA at 30 days, and about the same at 35 days (130), C57 tests yielded 83 per cent responses at 30 days, and only 8 per cent at 35 days. No reactions appear in Albino A and C_3H before three months. There is also an upper limit for sensitivity. Vicari (135) indicates this to be 80 days for DBA. Frings, Frings, and Kivert (37) report that DBA's convulse first at 18.7 days, C57's at 20.5 days, and mongrel Swiss Albinos at 20 days, with none of the between-groups differences significant. All three strains are said to be most sensitive before 30 days: DBA at 22, C57 at 24, and Albinos at 24 days. The DBA datum is at variance with Vicari's estimate, and probably reflects a difference in test procedure. It is clear that C57's are responsive if tested prior to 30 days. Dice and Barto's (24) discovery that *Peromyscus*, with an auditory range of from 500 to 95,000 cps, shows a decrease in auditory sensitivity with age may clarify these seizure results. It is also apparent that a certain level of neuromuscular development is prerequisite to convulsions. Frings and Frings (32) describe the movements of 12-13 day old animals as premonitory, yet they never culminate in an attack.

Strain, sex, and sensitivity. There is little among the rat data to indicate sex differences in susceptibility. The earlier mouse studies prompt the same conclusion (131, 142). But Vicari (132) has observed that endo-

crine therapy with several strains reduced seizure fatalities 40 per cent in males and 90 per cent in females. The protective effect of glutamic acid is also greatest for DBA males (41, 47). In line with this is Frings, Frings, and Kiverts' (37) conclusion that males are more fragile than females (14 male of 23 deaths in the DBA, two males of two in the C57 and three males of seven in the Swiss Albino sample). All of these studies extend the findings of Ginsburg and Hovda (48) with DBA animals. Meanwhile, the Fuller, Easler, and Smith (40) data also suggest that sex is related to susceptibility, its influence being controlled by the genotype. In highly susceptible or relatively immune stocks sex makes little difference; in stocks of moderate susceptibility it is an effective variable. When responses of offspring of nine possible combinations of DBA, C57 and their F_1 were evaluated, using first trial seizure risk as a criterion, males had a higher percentage of seizures in five comparisons, females in two and in two there was no difference. The largest differences were in the $F_1 \times C57$ back cross and in the F_1 hybrids. Additional support is provided by the higher incidence in $DBA/2 \times C47/10$ males as contrasted to $DBA/2 \times C57/6$ males (98).

The genetic mechanism of susceptibility. Speculation about genetic transmission in the rat has centered about three modes: single dominant gene, a pair of genes, and multifactor determination. The differences in theorizing reflect differences in empirical data that have been attributed to the use of impure strains (65) and inadequate control of other pertinent variables (27). The picture on homogeneous mouse strains also lacks complete unequivocality. From the proportions of *reactors* (animals giv-

ing at least one convulsion in a series of four tests), obtained with $DBA/1$, $C57/6$ and the offspring of crossing these lines, Witt and Hall (142) have concluded that mouse susceptibility is governed by a single dominant gene. However, their prediction for the critical back cross ($C57 \times$ non-reactors of $F_1 \times C57$) was not confirmed. This prompts the recognition of "minor modifying factors." Ginsburg and Hovda (48) hold the Witt and Hall conclusion, but point out that susceptibility varies with age. In a later experiment involving crosses of $DBA/2$ and $C57/10$ Ginsburg, *et al.* (50) obtained proportions in F_1 intermediate between those of the parent strains, and in F_2 three-fourths that of F_1 , and propose the presence of two or more nondominant factors in addition to a "convulsing" gene. Frings, Frings, and Kivert (37) reconcile their report of maximum sensitivity in DBA and C57 during the 20-day period with the Witt and Hall and the Vicari results for 30-day animals by suggesting that the two strains differ with respect to a single gene pair, the recessive allele producing only the early sensitivity, the dominant being associated with both the 20- and 30-day susceptibility.

Fuller, Easler, and Smith (40), noting the failure of the Witt and Hall (142) critical backcross and the divergence of results (50, 142) with different sublines of DBA and C57, tested the Witt and Hall hypothesis by reproducing their test procedure (except that seizure risk on first test rather than reactor-nonreactor classification after four tests served as the susceptibility index) with $C57/6$ and $DBA/2$. Two F_1 samples were intermediate between the parent strains, susceptibility appearing to decrease in accordance with the proportion of DBA genes in each group. Contrary to the single gene hypothesis, how-

ever, the proportions of F_2 and F_1 convulsing in the first trial were significantly less than expected. Otherwise obtained convulsion frequencies did not defy the assumption that a single gene produces high susceptibility in homozygous material but only moderate sensitivity in heterozygous. The possibility of multigene determination meanwhile is suggested by almost identical seizure risks in an F_2 of nonconvulsing F_2 Ss and the $F_1 \times C57$. The variability in the F_1 indicates susceptibility is a characteristic that fluctuates within a wide range about the average convulsive risk, nongenetic factors perhaps determining position within this range. This would be most important in heterozygous animals whose mean position on the susceptibility dimension would be closer to the threshold than that of homozygous strains.

Though susceptibility and the capacity for recovery are both inherited, they do not appear to be genetically linked. This last fact and the priming data are elaborated by Fuller and Williams (45) and Fuller and Smith (44) into a kinetic theory of seizures: the occurrence of a convulsive reaction depends upon the balance achieved between a gene-controlled, rapidly developing excitatory and a more slowly aroused accommodatory process. Strain differences in convulsive risk and latency reflect differences in the rates at which these two mechanisms reach critical functional level.

Age and Sex Differences

Little more need be added concerning these factors. Maier, Feldman, and Longhurst's (84) study of changes in seizure pattern segregated age from practice effects by testing animals daily and weekly over the same age range. They report that the rat's reaction on the first test is prognostic

of his responsiveness on later tests. They also found greater sensitivity in the animals tested weekly. Running fits and full epileptoid fits occurred predominantly in the early weeks of testing, a modified, controlled type of seizure frequently followed by hypertenseness becoming the predominant later response.

The effects of susceptibility produced by a 10 per cent lactose supplement differ for male and female rats (86). In males a decrease in the number of seizure-prone animals occurs without effecting the frequency of response in the reactors; in females the frequency of reactions decreases with no change in number of convulsions. Since the treated group displayed more epileptoid reactions than the controls, it was concluded that lactose enhances irritability in seizure-prone Ss, this change being more general in females. The smaller number of male reactors was attributed to a sex difference in the optimum proportion of lactose. However, since there are no data to indicate what the prelactose sensitivity of the experimental animals might have been, it is not improbable that the sex-lactose relationship is confounded by other variables.

Vicari, Tracy, and Jongbloed (138) have found that the administration of testosterone, pregnenolone, estradiol, cortisone, and cortin all alleviate convulsion and death to one degree or another. So also does glucose, epinephrine, and thiouracil. Ginsburg and Roberts (51) report sex differences in the effects of a variety of drugs upon the incidence of convulsions. Injections of glutamine aggravated seizures in male DBA mice, not females; methionine sulfoxide, and α -methyl glutamic acid had a greater influence in females; γ -glutamyl hydrazide depressed sensitivity in females, enhanced it in males; dinitro-

phenol depressed activity in males; and cortisone and γ -amino-butyric acid enhanced convulsions in females.

Differences in Physiological Status

Physiological changes during convulsion. Hofeld (69) describes a sharp increase in BMR during the audiogenic attack, with a drop to subnormal level in the comatose state following attack—all expected changes. Ginsburg and Hovda (48) claim a decrease centrally in free acetylcholine and a complementary peripheral increase. While there was no difference in free acetylcholine in brain extracts of untested DBA and C57 animals, the submaxillary gland extracts of the former contained significantly less than those of the latter. Aboud and Gerard (1) report defective phosphorus metabolism during the susceptible period: ATP-ase, alkaline phosphatase, and P/O ratio were all below normal as was liver glycogen, and there was a slow turnover of phosphorylated intermediates. Brains were lighter in susceptible strains. Cain, Mercier, and Corriol (14) describe the large waves in EEG records made during tests reported earlier by Lindsley, Finger, and Henry (76). These they regard as artifacts of vigorous movement. No other consistent change occurred save an occasional diminution in the large spikes during the passive phase. They thus conclude that audiogenic seizure lacks the electrographic characteristics of experimental epilepsy. Griffiths (61) more recently has obtained both slower heart and slower brain tracings during the attack, which, in contrast, he attributes to a reduction in movement (manipulative movement).

Prenatal environment and responsiveness. Hall (64) describes three convulsions and two deaths in three DBA fetal transplants into C57

mothers when tested at 28 days and suggests the importance of heredity as contrasted to prenatal environment in determining the constitutional bent of certain strains for convulsions. Ginsburg and Hovda (48), while confirming Hall on convulsiveness, found fewer deaths in DBA's born to and reared by C57 mothers than in nontransplants.

Dietary variation and responsiveness. Most of the studies in this category deal with the influence of the B vitamins upon susceptibility. Patton (109) has pointed out that while they do not explain convulsive activity, they demonstrate how the sensitivity of a seizure-prone, or potentially prone, subject may be intensified or alleviated. They thus offer a technique for a more precise experimental control over the dependent variable. Patton (113) extending his previous findings on the B complex, reports that young animals maintained on a basal diet including thiamin, riboflavin, pantothenic acid, choline, and pyridoxine display a high incidence of convulsions. The addition of five more vitamins: nicotinic acid, para-aminobenzoic acid, biotin, folic acid, and inositol; rice polish concentrate; ventriculin; or liver extract resulted in a marked reduction in convulsiveness. Less striking results occurred with supplements of brewer's yeast or with the feeding of Purina Chow. The animals on the basal diet were otherwise apparently healthy. Sensitivity can also be reduced by supplements of pyridoxin, if a deficiency of pyridoxin has been induced, this latter requiring feeding of the deficient diet from birth (120). In a cleverly conceived experiment, Griffiths (60) tested for susceptibility during two-week periods when his Ss were maintained on a stock diet and a self-selection diet. Three highly suscepti-

ble animals and three of low sensitivity ceased having fits while on the self-selection diet; five of these took in atypically large amounts of thiamine hydrochloride, the sixth, of magnesium chloride.

Several studies have dealt with convulsions in rats on sugar-supplemented diets. The Maier, Longhurst, and Ellen (86) study, already referred to, substituted 10 per cent lactose for cornstarch in the pregnant mother's diet and that of the offspring for nine months and resulted in an increase in sensitivity in the latter. However, according to Benedict (5), a 20 per cent lactose supplement from weaning until three or four months of age has no effect upon the frequency of reactions. The same is true for 20 per cent supplements of dextrose. Ginsburg and Roberts (51) also report supplements of dextrose to have effect with DBA/1. On the other hand, Vicari, Tracy, and Jongbloed (138) claim 5 and 10 per cent supplements of glucose to have a protective effect.

Bevan, Hard, and Seal (7) induced shorter response latency and greater frequency and intensity of seizure in young rats by feeding a lysine-deficient diet. A return to Purina Chow restored normal sensitivity. Animals maintained on the experimental diet also show high and more variable wheel activity, poorer maze performance, greater irritability, reduced food and water intake, loss of body weight, hypoglycemia, low hemoglobin level and red-cell count, lower total serum protein, and markedly reduced clotting times.

Hamilton and Maher (67) included a series of seizure tests in an experiment on the effects of glutamic acid supplements upon rat behavior. The number of seizure-prone animals, however, proved too few to warrant any conclusion concerning suscepti-

bility. Ginsburg, *et al.* (53) found that while supplements of glutamic acid had no effect upon seizure incidence in DBA mice, fatalities decreased. This protective action they attribute to the fact that glutamic acid is a nonessential amino acid. Since it is derivable from α -ketoglutaric acid, any excess of it may result in a higher cell-concentration of α -ketoglutaric and consequently an increase in energy output via the tricarboxylic acid cycle. The animal thus has energy enough to prevent severe spasms and/or to survive respiratory arrest. Later Ginsburg and Roberts (51) found not only reduction in deaths but also in seizures after 1+glutamic acid supplements. Fuller and Ginsburg (41) rejected the possibility that the protective effect of glutamic acid is due to adrenergic action after finding its persistence following adrenalectomy and adrenalectomy with DCA implants. The protective effect of glutamic acid in females may be further enhanced by using an interrupted rather than a continuous stimulation schedule (47). Other metabolically active substitutes also have a palliative effect; lactic acid, succinic acid, and γ -glutamyl hydrazide (51). Others, however, enhanced sensitivity or exerted no effect at all: malic acid, α -ketoglutaric acid, sodium pyruvate, isomolar injections of glutamine, methionine sulfoxide, α -methyl glutamic acid, and γ -amino butyric acid increased responsiveness; alanine, aspartic acid, fumaric acid, and sodium oxalacetate had no observable effect.

In the early part of the period covered by this review the effects of an induced magnesium deficiency were still under consideration. Lazovik and Patton (75) indicated that initially insensitive rats could be made to convulse after maintenance

on a low-magnesium diet and returned to normal insensitivity by magnesium sulfate supplements. The same was found to be true of hamsters (112), which convulsed even after subparalytic doses of curare. Both wild and domestic rats reacted after a magnesium-deficient regimen, although the wild were the more resistant and showed no fatalities (57).

Hormone and drug effects. Since the audiogenic seizure has been thought of in terms of emotional stress and emergency state, a vigorous interest has been maintained in the relation of drug and hormone balance to seizure sensitivity. Nicotine facilitates convulsions (38). Meanwhile, two studies (48, 51) indicate that subshock doses of insulin and two (23, 56) that alcohol have a protective effect. It has been suggested that every agent that reduces metabolic rate will protect from seizure. Thiouracil prevents death in DBA and reduces the severity of convulsions (136). Reduced body temperature is associated with immunity. This, in part at least, may be related to the epinephrine function. This should mean that overactivity of the adrenal gland would be associated with hypersensitivity. However, if the audiogenic seizure be considered an emergency state, increased output should have a desensitizing function, since it is associated with energy mobilization. Meanwhile, should the convulsion be a symptom of autonomic imbalance, any agent, sympathetic or parasympathetic, which restores that balance should have a protective effect. Three papers (12, 90, 138) describe the results of injections of adrenalin as alleviating. The same is true for certain sympathetomimetic substances: extract of genet (12), ephedrine (90), Benzedrine (90), ergotamine (93), and yohimbine (93). Mercier (93) is in-

clined to regard these drugs, particularly the last two, to effect sensitivity through a hypotensive action. In contrast, Chance (16) finds enhanced sensitivity in mice after doses of ephedrine and Benzedrine. This inconsistency is unexplainable unless it may be attributed to a difference in strength of dose or to the possibility that one worker used insensitive, the other sensitive animals, the drugs having no effect in either case.

Griffiths (59), attacking the hypothesis that lethargy and inactivity are associated with immunity, tested animals given a one-stage adrenalectomy. No differences in seizure incidence were found to obtain between operated and control animals, either emotional or nonemotional, as indicated by the Hall open-field test. Similar results occurred when Griffiths and Cohen (62) induced lethargy with morphine sulfate.

Yeakel, *et al.* (143) exposed rats to air blast for daily five-minute periods over the course of a year and produced an increase in blood pressure, and possibly an increase in adrenal weight, not found in unexposed rats. In a follow-up experiment (79) animals with elevated blood pressure were subjected to adrenalectomy and replacement therapy with the expectation that if hypertension had resulted from adrenocortical overactivity, blood pressure would return only to normal from the postoperative low; if not, replacement therapy would effect a return to the hypertensive state. The average initial systolic pressure for sound-exposed animals was 125; during the last six months of stimulation, 162; after surgery, 108; after replacement therapy, 125,—prompting the conclusion that air blast constitutes a chronic alarm stimulus and high blood pressure represents a symptom of hyperactivity of the adrenal cortex and the

failure of the organism to adapt. In contrast, Ginsburg and Roberts (51) report that cortisone raises seizure and death incidence in DBA/1. Hurder and Sanders (70, 71) failed to induce an anticipated change in incidence with injections of ACTH, although the susceptible-treated animals were found to have heavier adrenal glands than the unsusceptible-treated. Finally, Vicari, *et al.* (138) described a slight protective effect for cortisone and cortin that is in line with adaptation syndrome expectation. They also found other steroids (testosterone, pregnenolone, and estradiol) to be more effective than cortisone and cortin in reducing both incidence and fatalities.

Because of our present knowledge concerning the action of acetylcholine in autonomic ganglia, injections of this substance in appropriate form may be expected to influence—possibly enhance—sensitivity. Both acetylcholine and Eserine are reported to reduce incidence and fatality (49). Prostigmine and atropine were found to reduce deaths. In addition, there is a central decrease with a peripheral increase of free acetylcholine during seizures in DBA animals (48).

Since Tridione has been found to alleviate human *petit mal* attacks, as well as experimentally induced drug and electroshock seizures, its effect upon audiogenic convulsions has been explored by Adams and Griffiths (2). When given intraperitoneally to a small number of animals in doses comparable to that for the *petit mal* attack (3.7 mg/kg of 1 per cent solution), no effect upon the running attack was observed. When the dosage was increased to 10.8 mg/kg, incidence showed a marked drop. In no case did the drug appear to paralyze or effect confusion in the animals. Mercier and Cain (94) report the pro-

tective dose to be 300 mg/kg in 5 per cent aqueous solution of urethane given subcutaneously. This is considerably higher than the critical doses reported by these same workers (13) for diphenylhydantoin (20 mg/kg) and phenobarbital (3 mg/kg). These last are not toxic although that for diphenylhydantoin is about four times the critical dose for human beings. In their Tridione study, subjects became lethargic with doses of 20 mg/kg and suffered paralysis with doses of 40 mg/kg. Because of this protective effect of Tridione they classify audiogenic seizures as epilepsy. Adams and Griffiths (2), because of the difference in critical doses for rat and man, prefer to remain uncommitted on this point. Mercier (91) later investigated the effect upon seizures of introducing bromine into the phenobarbital molecule as well as the effect of bromine itself. Comparison of equally strong doses of phenobarbital and metabromogardenal indicated the latter to be slightly more protective. Doses of sodium bromide varying from 50 to 600 mg/kg and giving from 20 to 50 per cent protection were never as effective as metabromogardenal. Thus in the case of this last the barbituric nucleus is predominant. In another study (92) the effect of substituting an ethyl for a phenyl radical was explored. Diethylmalonylurea (barbital) was given in dosages comparable to the critical dose of phenobarbital and found to be less effective and less reliable. With double the critical dose, treated animals were unresponsive on about one third of the trials. However, since a complete epileptic attack was never induced, barbital was thought to have a sedative effect. Derivatives of the phenyl 5 group were also replaced by anisyl and piperidyl radicals to give dianisyl-5.5-hydantoin and

dipiperyl—5.5-hydantoin, but both substances appeared to be without anticonvulsant action. Thus the anticonvulsant effect of hydantoin derivatives depends on the integrity of phenyl 5.

Much of the work on hormones has involved nonphysiological dosages. Thus, while the results may be of pharmacological interest, they add little to our understudy of the relation of normal hormone balance to sensitivity.

Middle ear infection and convulsive sensitivity. In 1947 Patton (111) suggested that because of its common occurrence in albino Norway stock, purulent otitis might be considered to be a contributing factor to seizure sensitivity. While only two among a group of male and female breeders, 85 per cent of which were infected, showed signs of responsiveness, a high relation between infection and susceptibility appeared to exist among his young animals: about three-fourths were both infected and susceptible; only a small percentage were infected but nonsusceptible; none free of the disease had a convulsion. Kenshalo and Kryter (74) attempted to clarify this infection-convulsion relationship by using white noise as the seizure-provoking stimulus and a more sensitive infection criterion. After a series of auditory tests, the bulla was swabbed and a brain-heart media, inoculated with its contents, incubated for 48 hours at 37° C. The culture was then smeared on a blood-agar base and incubated for an additional 48 hours, each colony being finally stained with gram stain. Other cultures were made in nutrient media containing *d*-mannose. The criteria for infection were gram stain reaction, acid reaction to *d*-mannose, and microscopic appearance. Of 71 animals, 46 were found to be infected in both ears, 10

in one ear, and 15 healthy. Of the "two-ear" group, 85 per cent were reactors (one or more convulsions); of the "one-ear," 90 per cent; of the "no-ear," 80 per cent. Kenshalo and Kryter thus conclude that infection is not a necessary condition to convulsion, but may enhance susceptibility since the "two-ear" group convulsed on 81.5 per cent of the trials in contrast to the "no-ear" 36.3 per cent. Other data also indicate the nonessential role of infection (90). Of 25 DBA's subjected to histological examination after seizure test only three showed massive and one mild infection. Among 12 C57 animals, there were two massive and two mild infections. Marx and Jurko (88) doubting that Patton's infected rats were purulent, tested 82 animals, using several measures of sensitivity. Those found to be bilaterally diseased had displayed a greater proportion of convulsions than those unilaterally infected. Of 17 animals classed as insensitive, only two had both ears infected; of 23 classed as most sensitive, 17 had both ears infected. Infections, furthermore, were predominantly of the secretory type. The fact that 13 of 17 insensitives were diseased while 4 of 8 normals were sensitive reinforces the conclusion that middle-ear infection is not necessary to seizures. Marx and Jurko (88) suggest that infection exerts its influence by bacterial invasion of the inner-ear or brain or by lowering the threshold for extra-auditory pain. Frings and Frings (29) checked the possible relationship of infection to seizure in three strains of mice: DBA/1, C57/6 and mongrel Swiss Albino. While animals giving no seizure responses for at least 10 days prior to examination were free of infection, one of those dying in convulsion had otitis. Thus infection was judged to be unimportant for

convulsions in these animals. In contrast, Patton and Zabarenko (118) in an unpublished experiment involving 355 young rats, found the great majority of their susceptible animals to be infected.

Marx and Chambers (87) set about to clarify the role of otitis media by examining for possible changes in sensitivity subsequent to experimentally inducing pathological state. To accomplish this last, a plastic, butyl methacrylate polymer, dissolved in acetone was used to occlude one Eustachian tube in each experimental animal. All but two of the experimental animals showed suppurative infection, these two displaying a secretory disorder; additional infection was found in the untreated ears of six experimentals and four controls. Only three animals in the entire population, however, had convulsions during the study. Thus it was concluded that suppurative infection is not associated with susceptibility. These data, of course, provide no test of the possible importance of secretory infection, nor rule out the conclusion that infection enhances sensitivity in already sensitive animals.

The nervous system and audiogenic seizures. Clinical studies of hydration of the cerebral tissue indicate an enhancement of frequency and severity of human epileptic attacks. The same relation has been found to hold for electroshock and certain drug convulsions in laboratory animals. Auditory stimulation four hours after peritoneal injections of isotonic glucose (100 cc/kg), however, was ineffective in producing a single reaction in insensitive rats (95). Of 29 highly sensitive animals, only three gave convulsions four or five hours after treatment. This protective effect, furthermore, appeared to parallel the development of hydration, since two animals of another

sample reacted 30 minutes and one hour after injection, none at intervals of three and four hours. When animals were tested 24 hours after injection, only 55 per cent appeared to have regained their pretreatment sensitivity. From these data it was suggested that the mechanism is not the same in audiogenic and electroshock and carbiazol convulsions, but is also not due to conflict.

Brdar and Moyer (11) sectioned the IXth and Xth nerves in DBA×C57 hybrids and tested for seizure sensitivity on four consecutive days after operation. Although this cross is reported to be 95 per cent sensitive, none of the 19 glossopharyngeal animals gave signs of convulsing. The sole survivor of vagus sectioning was also immune. Meanwhile, of over a dozen animals subjected to sectioning of the XIth nerve and immune during the regular sequence of postoperative tests, a considerable number succumbed in tests administered on the sixth operative day.

Of the remaining papers to be summarized in this section, one deals with the effect of frontal damage upon seizures; one with destruction of the motor cortex, and two with interruption of afferent pathways. In line with the early report of Weiner and Morgan (139), Longhurst (78) describes a decrease in both frequency of seizure and number of susceptibles following partial frontal ablation. Seven animals with unilateral damage to the depth of the basal ganglia showed temporary cessation of convulsions, two with deep bilateral injury, apparently permanent immunity. Seventeen with superficial damage showed no postoperative change in sensitivity. Longhurst suggests that deep lesions were effective because they interrupted the motor processes distrib-

uting directly to the putamen. Patton (112) has speculated on the importance of the midbrain for seizures, since he found subparalytic doses of curare did not prevent attacks in animals rendered more sensitive by restricting magnesium intake. The enhanced sensitivity with cortical ablation observed by Beach and Weaver (4) might be explained in terms of cortical release, although with removal of 90 per cent of the cerebrum one might also expect damage to subcortical centers. Further testimony to the importance of subcortical centers is found in Morin and Cain's (102, 103) reports that attacks can be produced after destruction in the motor cortex and degeneration in the corticospinal tracts. Postconvulsive stupor and *cereae flexibilitas* are said to be associated with increased thresholds for certain types of stimuli and the abolition of postural corrective and cortical static reactions with the preservation of the reflexes of support and balance.

Two studies have dealt with the possible importance of proprioceptive inflow for sensitivity. Finger, Bice, and Day (28) produced vestibular dysfunction with a long-term regimen of streptomycin injections (134, two per day with doses ranging from .1 gm/kg) but failed to induce any difference in the number of animals convulsing or the frequency of seizures as compared to controls given either dihydrostreptomycin or an isotonic saline. Bevan and Hunt (8) sectioned the dorsal columns bilaterally at the cervical level and failed to produce changes in the number or severity of convulsions or differences in latencies for the first running attack. They did obtain, however, significantly greater latencies for the epileptoid phase and significantly longer running time in the operated animals. These last also displayed a greater

number of running attacks and spent a greater proportion of the interval between onset of the running and tonic-clonic episode in running. This suggests that kinesthetic excitation, assuming it to be relayed via the dorsal roots in the rat, is relevant for seizure induction. The rather limited effect of surgery could be due to the limited functional significance of this modality, to the possibility that proprioceptive inflow follows another route in the rat, or to inadequate experimental lesions.

Psychological Status

The widespread appeal of the Morgan interpretation of audiogenic seizures has resulted in little consideration, except by Maier and his students, of the possible significance of psychologically specified variables in the etiology of these convulsions. Lazovik and Patton (75) for example, point out that to 1947 there was no convincing evidence that conflict produced the attack. To clarify any possible suspicion of its significance, they created what they regarded to be a condition of maximum favorability for its operation: enhanced sensitivity due to magnesium deficiency. In an initial experiment, one group of deficient animals were given 10 trials per day in the jumping situation used by Maier to induce conflict and one daily sound test, a second receiving only the sound tests. Jumping was forced with electric shock. While the members of the first group typically developed resistance to jumping, crouched, failed to explore the jumping platform, and even squealed in the absence of shock, no seizures or prodromal signs were precipitated. Sound-test sensitivity was reduced with dietary-replacement therapy. A second experiment of the same general sort yielded the same results.

Maier and Longhurst (85) have refused to regard these experiments as a crucial test of the conflict hypothesis, maintaining that electric shock is a negative stimulus which the animal can localize and thus to which it can react adaptively. Consequently the "trapping effect" necessary for convulsion is not effectively induced. Their position is reinforced by data from a two-group experiment in which rats were trained in a discrimination task and then forced with an air blast to jump to the negative card. Controls, in contrast, received no discrimination training, but were forced to jump in the one-card test situation. The controls, during the course of testing, received, on the average, an air blast of slightly longer duration; however, forced jumping produced seizures in only 7.6 per cent of all tests as contrasted to 20.3 per cent for the experimentals. Similarly 37.8 per cent of the experimentals had convulsions in contrast to 9.1 per cent of the controls, the average number of seizures per animal (1.60) in the first group being significantly greater than that in the second group (.44). While this experiment cannot be regarded as a crucial test of the conflict hypothesis, it is regarded as a strong indication of the basic nature of the conflict interpretation. In a footnote in his 1949 book (83), Maier states that Morgan has withdrawn from the auditory reflex-conflict controversy on the basis of these last data.

Among currently active experimenters, Rabe (121) uses the sound test as a technique for inducing cumulative frustration. Observations of qualitative changes in the animals' test-cage behavior, and occasionally home-cage behavior, after a series of exposures to sound is consonant with Maier's view. A tractable animal, who initially explores the test-cage,

may come to resist handling or removal from the home-cage, squeals when placed in the test chamber, cringes, shivers, and acts as if "it expects something unpleasant to happen" (37). Also consonant with a psychological interpretation are Chance and Yaxley's (19) and Zener's (44) demonstrations of a stage of controlled hyperactivity in the seizure sequence. Slow-motion pictures made by the latter indicate that in the early stages of running, the reacting animal, upon approaching obstacles or the sides of the test-box, turns its head so as to take its collision on a shoulder.

Lindzey's study utilizing five strains of mice (77), though it indicates a positive relationship between emotionality and susceptibility, in contrast to previous reports for Hall's emotional and nonemotional rats, does not find the two traits to covary consistently in the various strain groups. Meanwhile, Griffiths has steadfastly regarded constitutional differences in temperament to be associated with differences in sensitivity. He reports (57) that wild rats on a magnesium-deficient diet are more resistant to convulsions. He points out that tame animals usually have smaller adrenals than wild; and that wild Norways, less susceptible than wild Alexandrines, are highly pugnacious and fierce. His adrenalectomy and morphine sulphate experiments (59, 62) are further attempts to demonstrate a relation between temperament and susceptibility.

Griffiths and Stringer (63) attempted to induce differences in emotionality by subjecting young animals to intense stimulation (loud noises, rapid rotation, extremes of temperature, electric shock) and checking for possible effects on seizure-sensitivity in adulthood. No differences were demonstrated between

the traumatized and the control animals. Rearing the animals in what the experimenters (9) chose to regard as different environments (6 ft. \times 9 ft. sheet-metal-lined room with sawdust floor; 2 in. \times 4 in. hardware-cloth squeeze boxes in the large room; 14 in. \times 9 in. individual cages) also failed to affect susceptibility. Tests in which rat Ss manipulated some aspect of the environment (61) resulted in immunity. When pretest experience was such as to reduce such manipulation, convulsions occurred.

Attempts to induce changes in sensitivity through specific training procedures have had one or the other of two aims: to provoke convulsions with initially ineffective stimuli, or to reduce the efficiency of stimulation already classifiable as seizure-inducing. Two experiments of the first sort have been unsuccessful. Coleman (21), investigating the convulsion as a fear reaction, tried to condition the convulsion to electric shock. While he failed to obtain a single convulsion in eight Ss, seven of these animals came quickly to show "substitute" behavior—preening, vibrissae-twitching, crouching, etc. In a similar experiment Goldberg (54), using an intense flashing light as the conditioned stimulus, observed no tonic-clonic episodes when the light was presented alone. Several running responses were initiated during the presentation of the light but their significance is not clear. Exploration, which had increased, and crouching and preening, which had decreased, during a preliminary phase in which the light was presented without the bell, decreased and increased respectively after the light had been paired with the bell. Frings, *et al.* (37) report mice to behave apprehensively, as if "something is going to happen."

Experiments on learned control of

the seizure have been more successful. Marx and Van Spankeren (89) trained rats to cut off the sound by pushing a pole and then delayed making the pole available in order to determine if control of the stimulus also meant control of the seizure. While there were no significant differences in the number of running attacks and convulsions during the training sequence, the trained had reliably fewer attacks of both sorts during the period of pole-delay testing. During a period of extinction, the trained animals showed a shift back toward their pretraining sensitivity level. Additional indications of control were found in the forepaw clonus that emerged during training and in the attacks that sometimes occurred after the animal had executed the pole response. The experimenters suggest that the animal subjects develop a technique that releases them from a noxious situation and with it a "sense of control" as indicated by a general resumption of eating behavior. In a further study, Goodson and Marx (55) substituted wheel-turning for pole-tilting. Members of a control-experimental pair were testing simultaneously in adjacent test chambers. Both were subjected to auditory stimulation and electric shock through the feet. The experimental animal, however, had available in its compartment a wheel which, when turned, reduced the strength of the shock current. Comparison of the two groups of Ss on a seizure-resistant index indicated the experimental animals to be reliably more resistant. When the control animals were given wheel training and the experimentals denied access to the wheel, the resistance relationship was reversed. These results are in line with Griffiths' previously described conclusions on manipulation.

Chance (89) meanwhile reports

that the introduction of a plastic hut for a brief period at the beginning of each test provides for immunity. Three of his mice showed searching movements when the hut was not present, creeping to the area where it had been, then sitting relaxed through the sound test. In order to check on the possibility that the hut reduced stimulus intensity, a plastic horseshoe was substituted for the hut with no change in results. This learned refractoriness is regarded as responsible for the wide intra- and intersubject variability in susceptibility. It most readily supports the general class of psychological seizure theories.

AUDIOGENIC CONVULSIONS AS AN INDEPENDENT VARIABLE

Some Effects upon Bodily Processes

A few papers describe more lasting physiological changes than those previously associated with the period of testing and/or convulsion. Patton, Russell, and Pierce (117) report increased sensitivity to electroshock convulsions following a series of daily exposures to subconvulsive auditory stimulation. Yeakel, *et al.* (143) exposed rats to five minutes of subconvulsive air blasts daily for one year and at random intervals thereafter and report persistent hypertension in the subjects during the last two to four months of treatment. During the first two months the mean systolic pressure in their controls was 113 in contrast to the experimentals' 124 mm. Hg., during the last two months 127 in contrast to 154. While no morphological change was apparent in the adrenal glands of the blasted animals, their average gland weight tended to be greater. In a second experiment (79) adrenalectomy was followed by slightly subnormal blood pressure (97 mm.

Hg.), replacement therapy restoring the normal level.

To investigate possible seizure-connected pituitary dysfunction, Nolan (107) examined virgin female rats for signs of pseudopregnancy during a series of 21 audiogenic seizure tests. No change in length of oestrus cycle was observed in the convulsing experimentals, nonconvulsing experimentals, or unexposed controls. Exposure to electroshock convulsion also failed to induce the condition, a result at variance with the previous report of Jensen and Stainbrook (73). Ginsburg and Huth (49) have found less free acetylcholine in the submaxillary glands of DBA mice than in C57's. No differences, however, were found in the acetylcholine level in whole brain extracts of these strains.

Death during seizures appears as a risk in rats only if they have been subjected to severe nutritional deficiency or are very young. That it is, in contrast, a common outcome with certain strains of mice has already been discussed.

Postseizure Changes in Behavior

Reports of changes in emotional tone subsequent to a series of seizures continue to appear in the literature (8, 54). Rats initially gentle and curious, after only one or two exposures, begin to display premonitory symptoms when placed in the test chamber. Exploratory behavior ceases, the animal crouches, shivers, and displays weaving head movements. He may become more inactive and "retiring" but more irritable and jumpy and resist being removed from his home-cage and/or placed in the test situation. Similar changes have been described for mice (37). In addition, nonconvulsing rats are characterized as more compulsive (82). James and Boyles (72) found a

significant reduction in food and water intake in rats following convulsion, normal levels being restored within 58 hours. This they attributed to the temporary drop in activity that generally follows convulsions.

Maier (82) claims that convulsing rats breed less readily, a condition that might be expected to accompany a state of chronic anxiety. Still there are no more recent data to contradict the findings of Farris and Yeakel (25, 26). Frings, Frings, and Kivert (37) compared litter size and survival in three mouse strains: mongrel Swiss Albinos produced large litters and reared most to testing age, C57's had smaller litters but reared most, DBA's had the fewest offspring per litter and reared only two thirds of these.

Hall and Whiteman (66) exposed DBA×C57 hybrids to four sound tests at the age of four to seven days, then obtained measures of emotionality with the open-field test at 30-40 days, the stoyepipe test at 70-80 days, and the open-field test again at 100-110 days. The experimentals were judged more emotional than controls not exposed to infantile stimulation, but no reliable difference was apparent after the first test. This was attributed to familiarization with emotionality-test situations. Challenging these results Griffiths and Stringer (63) exposed rats to auditory stimulation, rapid rotation, shifts in temperature, and electric shock until 57 days of age. At 60 days observations were begun of behavior in the open-field situation, susceptibility to audiogenic attack, and performance in an 11-blind Warner-Warden maze and Lashley jumping apparatus. At no time were there significant differences between the experimental animals and unexposed controls on the tests.

Chance (17) calls attention to

what he refers to as a new component of the postconvulsive behavior of *Peromyscus*. While testing mice in pairs, he observed that the first animal to recover from the epileptoid phase frequently attacked its test partner, holding with its forepaws and vigorously biting. Such attacks, however, were also directed toward a stuffed mouse pelt, a cotton-wool roll, and a plasticine model, the roll and the fur being attacked preferentially. The response is well coordinated and hence regarded as an automatism. The present writer has occasionally observed similar behavior in rats when attacks were followed by hyperexcitement.

A small number of papers deal with possible effects of exposure to intense sound and/or convulsions upon performance in routine learning situations. While these serve to clarify certain previously reported relationships they add little that is new. Brady, Stebbins, and Galambos (10) trained rats to make a lever-pressing response and then conditioned an emotional response (suppression of the lever response by a combination of click and shock). After the sixth conditioning trial, a sequence of auditory tests, 21 on alternate days and 15 daily, was begun. After a three-day interval retention of the conditioned emotional reaction was checked by giving one nonreinforced trial. This last was almost completely abolished in animals with 30 or more seizures, markedly reduced in animals giving from 1 to 26 convulsive reactions, but suffered no change in seizure-immune animals. The rank-order correlation between number of convulsions and an index reflecting strength of lever pressing response was .89, indicating the importance of convulsions, rather than exposure to sound, for performance.

Different conclusions are suggested by Shaw, Utecht, and Fullanger's (126) experiment. While both studies demonstrate a clear difference between convulsives, on the one hand, and nonconvulsive and nonexposed controls, on the other, the latter experiment does not indicate defective retention to be responsible for this. Thirty-seven rats (16 controls, 12 nonconvulsing and 9 convulsing experimentals) were trained in a Tolman, Ritchie, and Kalish place-response maze, modified by attaching tunnels to the alleys near uncovered food boxes. The experimentals were then exposed to 5 minutes of key jingling. After 25 minutes three retest trials were run at 2-minute intervals. While there were no differences among the three groups of *Ss* on initial learning, the convulsers took markedly longer on the retest trials. There were, however, no between-groups differences in the mean numbers of errors during the retest sequence. None of the animals hesitated at the choice-point. The convulsives, meanwhile, "hid" in the tunnel, only two of the nine going on to the food box on any of the 10-minute retest trials. This failure to go to food after correct choice is explained as the superposition of a fear response upon a learned pattern and is hard to reconcile with the Brady interpretation of poor retention due to the disorganization of new traces resulting from violent convulsive activity.

Similar to the Shaw, Utecht, and Fullanger results are those of Stanibrook (129) who reported that 30 three-minute sound tests, yielding an average of 62 per cent convulsions, were followed by prolonged times on at least five trials of relearning a maze. There was, however, no increment in errors. The behavior of the rats on the first several relearning

trials was characterized as hyper-vigilant and sensitive to fright. In a further experiment utilizing a water maze, "noise-fright" rats most frequently displayed VTE at the last learned choice-point, suggesting that the disrupting effect of fright is greatest upon more recently acquired reactions. Thirty days after sound tests were terminated, no detrimental influences were observed in maze performance. In line with this and the results of Griffiths and Stringer (63) previously described, Gilbert and Gawain (46) and Martin and Hale (105) find no clear evidence of decrement in Warner-Warden maze performance for nonseizure rats exposed to sound varying from 5.5 to 33 kc in frequency, 110 to 147 db in intensity, and up to 15 minutes in duration.

Shohl (127) reports no effect of exposure to sound *per se* upon acquisition of a brightness-discrimination habit when comparison involved conventional time and error criteria. However, qualitative differences were apparent in the behavior of certain animals on exposure days: they displayed jerky running, exaggerated hesitation, and posturing. There were, in addition, increased errors, longer starting times and marked position habits on the days during which the subject had convulsed. Generally, position habits appeared only after the animal had had several convulsions. Since poorer performance occurred only on convulsion trials, Shohl suggests the disturbance to be due not to learning, but to either severe emotional upset or temporary metabolic defect. In summary, these few studies prompt the conclusion that defective performance, when it is associated with convulsiveness, derives from a generalized but transient noncognitive change, not from a faulty capacity for learning or retention.

TRENDS IN INTERPRETATION

Not all animals in an experimental population succumb to convulsion-provoking auditory stimulation. Nor do sensitive individuals respond on all occasions. The appearance of a convulsive response, and the form it takes, is contingent upon a variety of factors: the nature and duration of the auditory agent, the application of drugs, the degree of restraint placed upon the animals' movements and/or the extent of manipulatory activity, the animal's strain, its nutritional status, its body temperature, lesions in its nervous system, what it has been trained to do in the test situation, the presence of middle-ear infection, and possibly its sex, hormone balance, and temperament.

Explanations of seizures have been delineated within both psychological and physiological frameworks. Physiological theories, in general more readily accepted, have derived from observed changes in sensitivity associated with characteristics of the auditory stimulus, binding or otherwise restraining the animal's movements, variations in dietary intake, the administration of drugs, and surgical damage to the nervous system. They are most appropriate for the data on strain and age differences in susceptibility, interanimal similarity in seizure pattern, and the tendency for responses to occur within about 60 seconds. In their present form they cannot effectively deal with increased susceptibility following conflict or control through learning.

The oldest of the physiological theories (100) maintains that the audiogenic seizure is an overflow phenomenon: severe auditory bombardment effects a high level of excitation in auditory centers with resultant spread into adjacent motor centers and widespread reflexive re-

action. Consonant with this interpretation is the superiority of noise over pure tones, high auditory frequency and intensity over low frequency and intensity, the data on priming, and the presence, almost without exception, of sound with the occurrence of convulsions. The effectiveness of binding in reducing seizure is attributed to straining against the bonds with the consequent reduction of motor tension before it reaches a convulsive level. Inconsistent with this is the immunity of completely immobile animals and the inevitable running that presages the tonic-clonic attack.

Though intense sound be assumed to be a necessary condition to seizure—and this is yet to be unequivocally established—the overflow model need not be the simple one just described. A more elaborate model meanwhile requires more precise specification of the role of auditory excitation. Several possibilities present themselves: critical level overflow may originate in more than one sensory center; auditory input may serve to detonate intraorganismic changes that in turn lead to overflow. Experimental check on the vestibule as a source of positive feedback has yielded negative results (28), although data on the latency of convulsion and the duration of running in animals with dorsal hemisections suggest kinesis as a contributory factor (8). The work on middle-ear infection points to pain as an important modality, though direct experimental evidence is still lacking (88). Meanwhile, in recent years, the importance of the autonomic system and subcortical centers, has become increasingly clear. The few data available on cerebral dehydration, damage to the frontal areas, motor cortex and pyramidal tracts, sectioning of the ninth, tenth, and eleventh cranial

nerves prompt extension of the overflow model in terms of the extra-pyramidal system.

Audiogenic seizures may also be characterized as a depletion phenomenon, for they are precipitated most readily in animals suffering from dietary restriction or reduced oxygen tension. There is, however, no fundamental opposition between this and the overflow hypothesis, for overflow is a function not only of magnitude of stimulation but also of sensitivity of the nervous system and any condition or conditions that result in enhanced sensitivity make for overflow, or faulty operation of a governor that regulates overflow. The existence of genetic differences and the data on priming suggest that the defect may be of a rather specific nature such as is found in the relationship between phenylpyruvic acid metabolism and oligophrenia. Already a detailed model has been projected dealing with the protective effects of glutamic acid supplements.

Two bits of data warrant further exploitation: the claim that death in DBA mice results from respiratory failure, and reduced clotting times in susceptible animals. It may be highly important to determine if this latter effect is peculiar to the blood or occurs also in other body fluids. Should clots occur during the period of vigorous running, they could conceivably render the cerebrum anoxic and release the brain stem from cortical control with resultant convulsion. This speculation requires assuming that auditory input serves a detonator function. This has yet to be investigated.

Despite general preference for a physiological model, nothing has been done to assay systematically physiological differences among susceptible and nonsusceptible animals. Although electroshock convulsions

have been discussed within the framework of Selye's adaptation syndrome hypothesis (122), and although audiogenic seizures have been regarded as a stress phenomenon by both physiologically and psychologically biased investigators, no vigorous attempt to exploit Selye's logic has been made for the latter phenomenon. Meanwhile, Bevan (6) after repeatedly placing animals in laboratory situations which he characterizes as "conflict" and "fear provoking," reports changes in hemoglobin level, platelet and erythrocyte count, spleen size, and the presence of mucus and blood in feces and urine. The success of Williams and his colleagues (140) in defining "metabolic personality" in alcoholism, schizophrenia, and feeble-mindedness recommends his experimental approach to the student of audiogenic seizures. Another lead that might be profitably followed involves investigating the effect of systematically varying the chemical properties of convulsants and anti-convulsants.

The original theory of audiogenic seizures was a conflict theory derived to account for the convulsions observed in some rat *Ss* when forced with an air blast to respond in the Lashley jumping situation (81). According to Maier (81) the animal, trapped between two negative incentives, is rendered immobile by the simultaneous arousal of incompatible response tendencies. This results in the accumulation of tension, which if it reaches "catastrophic" level, induces convulsion. This is an overflow model elaborated in both physiological and psychological terms. The noise-fright, pain, emotionality, and escape hypotheses may all be interpreted as versions of it. It can deal with the enhanced susceptibility in sound tests that follows forced response in the one-card jumping

situation; the protective effects of "directed" activity (substitute behavior, conditioned wheel-turning, manipulation of binding or harness, escape into plasticine horseshoes, etc.); apprehensiveness in the test situation; and increase in frequency and change in pattern with repeated tests. It explains why sound is peculiarly effective (although it might be pointed out that this of all modalities requires the least energy for arousal). While it can account for the facilitative effects of priming, it cannot explain the protective effects nor are certain of the more recent data on reduction of movement consonant with it. As was stated earlier, it cannot deal directly with the possible effects of middle-ear infection, damage to the central nervous system (although it is not inconsistent with data suggesting the importance of the extrapyramidal system), and the clear-cut age and strain differences in sensitivity.

A purely psychological conflict model may be substituted for the psychophysiological model just summarized. The convulsive response is then viewed as a positive solution to the problem of escaping a distressful situation (i.e., as an hysterical convulsion) and thus a form of *aus dem Feld gehen* behavior. This interpretation might account for reduced seizures following substitute behavior, running to the locus of the hut, wheel-turning, manipulation of binding and total immobility, the controlled phase in the running sequence, the peculiar effectiveness of auditory stimulation, and the decrease in latency with continuous tests. It would suffer, however, the same inadequacies as the conflict-overflow hypothesis.

Controversy at this time constitutes indulgence in "metascience," for it requires almost complete de-

pendence upon emotional bias for resolution. This is so for a number of reasons. Any inference of functional principles requires a knowledge of independent variables, defined as dimensions. Our present information about audiogenic seizures is, for the most part, simply a listing of conditions which have been found to be associated with susceptibility. Among these data there is much that is contradictory and ambiguous. Furthermore, the indices used to describe susceptibility are rather crude and little has been done to devise more refined measures.

There also appears to be a prevalent assumption that alternative models must be in opposition; if a physiological explanation is adequate for certain data, the possible appropriateness of a psychological theory is excluded. Such an assumption is not valid, for the adequacy of a model is defined not by the number of alternative explanations it eliminates, but by its versatility in integrating data and its fruitfulness for prediction. That models may complement each other is suggested on the one hand by the behavioral data that yields the label, noise-fright reaction, and, on the other, by physiological data that indicates the importance of the extrapyramidal system.

However, in the instances where both extant physiological and psychological models seem appropriate, it is not possible to achieve unambiguous definition of the key concepts. While sound has been a component of every situation in which audiogenic seizures have occurred, it is not possible to describe a sound test which might not also be characterized as a conflict situation.

If available models lack definitiveness, they also are probably overly simple and over-extended. Although

it may be established that auditory stimulation is prerequisite to convulsions, it does not necessarily follow that auditory overflow is the crucial condition. Nor because conflict may be construed to be associated with premonitory behavior or the initiation of the running-phase does it mean that it explains the occurrence of the fit. We have yet to consider seriously the possibility of different mechanisms directing different phases of the response—the recent work on convulsive risk, death risk, and priming are exceptions. Perhaps a valuable dictum might be derived by modifying Beach's (3)

principles of multiple causation and multiple effects: in no instance is a response due to one and only one factor; nor does any single process or condition have one and only one behavioral result.

Audiogenic seizures constitute an important research problem. For the psychologist they provide a dramatic behavior phenomenon for study, for the neurophysiologist, a situation that may lead to important insights concerning central nervous system function, and for the biochemist, a potentially valuable assay technique—for all, an opportunity for interdisciplinary fertilization.

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SOME PERSPECTIVES ON "THE ATTENUATION PARADOX IN TEST THEORY"

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Dr. Loevinger (5) has performed a real service in focusing attention on certain of the earlier results (e.g., 1, 3, 4, 12) appearing in the literature regarding the relation of test "validity" to the item intercorrelations and to the distribution of item difficulty. The present note is intended to call attention to four known results that provide further clarification of the so-called paradox and of its effect on the selection of items in test construction.

Loevinger's "paradox" centers about the fact that, in a peaked test (a test composed of items all of equal difficulty), increasing the item intercorrelations beyond a certain point will lead to a decrease in a certain coefficient to which several writers have attached the term "validity." "Validity," in this special sense, is the product-moment correlation of test score with the common factor of the test items.

CURVILINEAR VALIDITY

As Brogden pointed out (1, p. 207), and as has been discussed elsewhere in some detail (6, 7, 10), the relation of test score to the common factor of the test items is necessarily curvilinear. This curvilinearity is commonly negligible for most tests in ordinary use, but it is not negligible (6, pp. 19-21) in the "region of paradox" with which we are here in part concerned—the region where the item intercorrelations are extraordinarily high. Consequently, in this region, the so-called "validity" coefficient is not an adequate measure of the virtue of the test. Obviously

the use of the usual product-moment correlation coefficient tends to underestimate the true degree of relationship whenever the relation is curvilinear. What is required is a coefficient representing the curvilinear correlation between test score and the common factor of the test items.

The writer (7) reported a study, parallel to Brogden's, showing the effect of item intercorrelation and of distribution of item difficulty on the curvilinear correlation of the test score on the common factor. The results showed that no paradox existed when this coefficient was used to evaluate the test. However, as the writer pointed out, it would have been preferable to consider the curvilinear correlation of common factor on test score. This was not done because of the severe computational difficulties.

Brogden (2, p. 378) subsequently suggested in this connection that the "paradox" would have been found to exist if the preferable curvilinear coefficient had been used. Tucker¹ has recently shown it to be obvious that in the extreme case of perfect item intercorrelations, the curvilinear correlation of common factor on test score will necessarily be reduced by peaking the test. Thus the "paradox" does exist, even when an appropriate measure is used for the "validity" of the test over the whole group of examinees.

Cronbach and Warrington, in one section of their study (3, pp. 145-

¹ Personal communication, February 1954.

146) actually computed the preferable type of curvilinear correlation (η) for most of their tests. Their results confirm the existence of the "paradox." When η is used, the peaked test is shown to be preferable over the unpeaked test for higher values of the item intercorrelations than would have appeared to be the case if the inadequate product-moment correlation had been taken as the criterion.

DOES THE PARADOX ARISE IN COMMON PRACTICE?

A possible misinterpretation (in no way attributable to Loewinger) of Cronbach and Warrington's important article (*op. cit.*) will now be considered. These authors present results for five different levels of item "precision," denoted by σ_d . It is not always realized that only the last of these ($\sigma_d=2.0$) lies comfortably within the range where aptitude- and achievement-test items commonly occur. Cronbach and Warrington give a table showing the interitem tetrachoric and phi coefficients corresponding to their values of σ_d ; however, many test writers do not commonly deal with either of these coefficients.

On the assumption that all items are free-response items of 50-per cent difficulty and that all item intercorrelations are the same, Table 1 shows for each value of σ_d the corresponding value of (a) the reliability coefficient (r_{11}) of a 100-item test (the split-half and the usual Kuder-Richardson coefficients are all equal when the items are statistically equivalent, as here assumed), (b) the tetrachoric item intercorrelation (r_{ij}'), (c) the interitem phi coefficient (r_{ij}), (d) the biserial correlation (R_i) between item and common factor, (e) the point-biserial correlation (r_{ia}) between item and common factor. Some of the less well-known relations between these coefficients for the present case are as follows:

$$\sigma_d = \sqrt{1 - R_i^2 / R_i},$$

$$r_{ij}' = R_i^2,$$

$$r_{ij} = \frac{2}{\pi} \sin^{-1} r_{ij}',$$

the last formula requiring the use of radian measure.

A glance at the reliability coefficients in the first row in the body of Table 1 shows that we seldom deal with tests having a value of σ_d much

TABLE 1
RELATION OF σ_d TO TEST RELIABILITY AND TO THE CORRELATIONS OF THE
ITEMS WITH EACH OTHER AND WITH THEIR COMMON FACTOR

	Value of σ_d				
	0	.2	.5	1.0	2.0
Reliability of a 100-item test (r_{11})	1.00	.998	.993	.980	.936
Item tetrachoric intercorrelation (r_{ij}')	1.00	.96	.80	.50	.20
Item fourfold point intercorrelation (phi) (r_{ij})	1.00	.82	.59	.33	.13
Point-biserial correlation between item and common factor (r_{ia})	.80	.78	.71	.56	.36
Biserial correlation between items and common factor (R_i')	1.00	.98	.89	.71	.45

smaller than 2.0. (Readers who are accustomed to working with item-test correlations may correctly assume that the last two item-common-factor correlations in the last two rows of the table are virtually the same in numerical value as the corresponding familiar item-test correlations.)

Cronbach and Warrington's data for η show that "paradoxes" occur, even in peaked tests, only when σ_d is less than about .75. It is thus seen that the paradox does not often appear in ordinary work with aptitude and achievement tests.

Is "VALIDITY" A BASIC CONCEPT?

Loevinger asks if it is not intuitively valid to demand that the basic concepts of psychometrics shall have a nonparadoxical relation to "validity." The present writer suggests that any over-all coefficient of validity for an entire group, even if properly measured so as to take curvilinear relationships into account, is from a theoretical point of view a mere hodgepodge, at least in the "region of paradox." One reason for this statement is the serious lack of homoscedasticity in the scatterplot that corresponds to the "validity" coefficient. When the item intercorrelations are high, the standard error of measurement is very different for test scores at different ability levels (11; 6, pp. 13-16; 10, pp. 533-536; 8).

A more basic concept than validity is the discriminating power of the test at various ability levels. From the viewpoint of the mathematical statistician, a test score is a sample statistic that is to be used to test some hypothesis or (what amounts to the same thing) to reach some decision. The crudely stated hypothesis may be, for example, that the examinee is suitable for college work. The examiner has set up some more

or less arbitrary rule for decision-making utilizing the test score as a guide. If the problem is properly formulated, all of modern theory of decision-making and theory of estimation and testing hypotheses can be brought to bear. It is possible, for example, to compare various psychological tests in respect to the statistical power of any specified decision procedure. An illustration of this approach is given in (8).

The discriminating power of any hypothetical psychological test with respect to any specified simple decision regarding any single examinee can be determined theoretically. An obvious index of this discriminating power would be simply the probability of reaching a correct decision regarding the given examinee (8, section 6). Another good index, in accordance with modern statistical theory, is the standard error of the maximum-likelihood estimate of the examinee's ability (8, sections 3 and 4).

It is too much to ask that the different discriminating powers of a single test for a wide variety of examinees and with respect to various unspecified decision problems should all be harmoniously and nonparadoxically summarized in a single average "validity" coefficient. Actually, if anything at all is known about the use to which a test will be put, it is usually apparent that different degrees of discriminating power are needed for examinees at different levels of ability. The test constructor's goal should be to achieve these various desired degrees of discriminating power rather than to maximize any single composite validity coefficient.

In actual practice, the discriminating power of a test at different ability levels might well be summarized by a diagram, as in Figure 1, where the

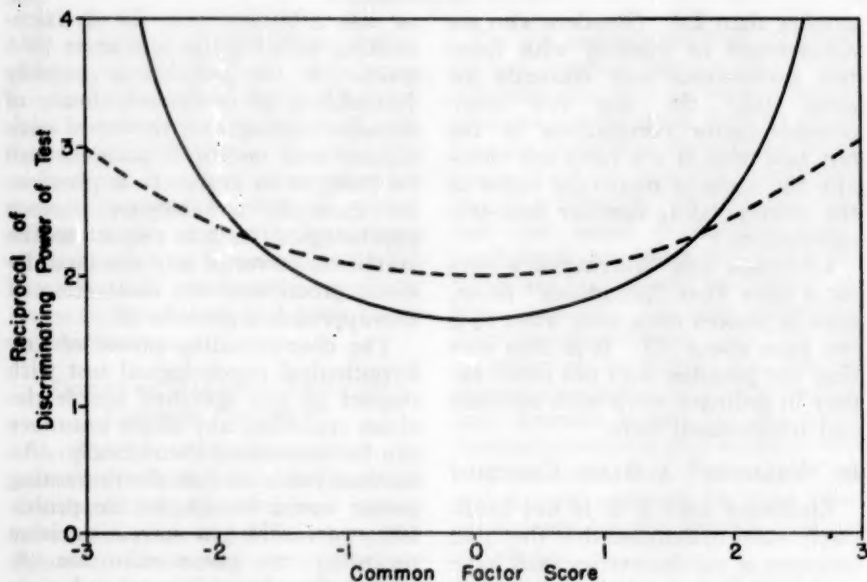


FIG. 1. THE DISCRIMINATING POWER OF TWO HYPOTHETICAL TESTS AS A FUNCTION OF THE TRAIT UNDERLYING THE TEST

ordinate is the reciprocal of the discriminating power. Here, the solid line represents a peaked test with items of 50 per cent difficulty and $R_i = .60$. The numerical values for plotting the curve were obtained from Table 1 of (8). The dotted line represents a (hypothetical) unpeaked test. The test constructor must decide in each instance what distribution of discriminating power best suits his purpose. Improved practical methods for preparing such curves for actual tests are in the process of development.

A MISCONCEPTION REGARDING PEAKING

Deplorably little has been written regarding the discriminating power of a test at specified ability levels. The present section is included in order to illustrate the theoretical importance of this concept, at the same time dispelling a rather serious misconception.

It is known that if a test is to be used only for discriminating between successful and unsuccessful examinees, then all items should be of the same difficulty level such that 50 per cent of the examinees near the cutting score answer each item correctly. This conclusion holds no matter how high the item intercorrelations are, and irrespective of any "paradox."

The misconception (not attributable to Loevinger) is that for optimum results the percentage of correct answers to each item should equal the percentage of examinees to be selected. Actually, (6, pp. 26-27) this misconceived rule can be correct only if the percentage of examinees to be selected is 50, or if each item is perfectly correlated with every other. Since interitem correlations (ϕ coefficients) are commonly nearer to .10 than 1.0, this rule will often be quite inadequate.

In order to determine the optimum

item difficulty for a test to be used for selecting a specified proportion, P , of the examinees, the following procedure is suggested (the procedure is outlined in 9; the theory from which it obviously follows, in 6, *loc. cit.*):

1. Convert P to a normal relative deviate, c .
2. Multiply c by the average item-test biserial correlation, denoting the result by h .
3. Find p , the area lying above the normal relative deviate h .

p is the desired optimum item difficulty (proportion of correct answers) for free-response items. A correction for guessing should be made if multiple-choice items are to be used.

Table 2 presents the optimum values of p for $r_{it}' = .30$ and $r_{it}' = .60$,

TABLE 2
OPTIMUM ITEM DIFFICULTY FOR A PEAKED
TEST HAVING SPECIFIED ITEM BISERIALS

Proportion of Examinees to Be Selected (P)	$r_{it}' = .30$	$.60$
.001	.18	.03
.01	.24	.08
.05	.31	.16
.10	.35	.22
.20	.40	.31
.30	.44	.38
.40	.47	.44
.50	.50	.50
.60	.53	.56
.70	.56	.62
.80	.60	.69
.90	.65	.78

the values of r_{it}' being chosen as slightly beyond the usual limits of the range of average item-test correlation coefficients for aptitude and achievement tests. At such levels, the item-test and item-common-factor correla-

tions differ very little from each other. A comparison of the second or of the third column with the first displays the error that would be made by following the erroneous rule mentioned at the beginning of the present section.

SUMMARY

Four points have been brought forward:

1. The usual product-moment "validity" coefficient is inadequate. A curvilinear correlation coefficient must be used. The "region of paradox" is still found when the correct coefficient is used, although its size is reduced.

2. A greatly magnified notion of the extent to which the "paradox" occurs in actual achievement and aptitude testing is frequently encountered. Some numerical values are tabled so as to clarify this point.

3. It is pointed out that any overall "validity" coefficient for a group of examinees can be no more than an average of heterogeneous information. It is not surprising if this inhomogeneity produces so-called paradoxes. Such a composite coefficient is not basic to psychometrics. The discriminating power of the test for a specified decision problem regarding a specified examinee is the truly basic concept. A very simple index for describing this discriminating power is defined.

4. One instance where a consideration of the discriminating power of a test at a particular ability level leads to some practical conclusions (irrespective of the "region of paradox") is mentioned, and a common misconception is corrected. A table is given showing the optimum item-difficulty level for the peaked tests required when the examiner's purpose is merely to classify a group of examinees as successful or unsuccessful.

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SPECIAL REVIEW

SOME RECENT TEXTS IN EDUCATIONAL PSYCHOLOGY

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BLAIR, GLENN M., JONES, R. STEWART, & SIMPSON, RAY H. *Educational Psychology*. New York: Macmillan, 1954. Pp. xvii+601. \$4.75.

COMMINS, W. D., & FAGIN, BARRY. *Principles of educational psychology*. (2nd Ed.) New York: Ronald Press, 1954. Pp. xvi+795. \$5.75.

CRONBACH, LEE J., *Educational psychology*. New York: Harcourt, Brace, 1954. Pp. xxvii+628. \$5.50.

MURSELL, JAMES L., *Psychology for modern education*. New York: W. W. Norton, 1952. Pp. x+610. \$3.90.

REMMERS, H. H., RYDEN, EINAR R., & MORGAN, CLELLEN L. *Introduction to educational psychology*. New York: Harpers, 1954. ix+435. \$4.00.

SMITH, HENRY P., *Psychology in teaching*. New York: Prentice-Hall, 1954. Pp. xiii+466. \$4.95.

SORENSEN, HERBERT, *Psychology in education*. (3rd Ed.) New York: McGraw-Hill, 1954. Pp. x+577. \$5.00.

All psychologists have a personal stake in educational psychology. In the first place, the status and prestige of psychology as a profession depend to an uncomfortable degree on its effectiveness in the field of education. Almost all candidates for teaching certificates are required to take at least one course in educational psychology and a large percentage of them take no other psychology

courses. To most members of this large and influential group, psychology means the educational psychology course they took in college and their attitudes toward psychology or toward psychologists will depend on the effectiveness or ineffectiveness of this course. The fact that educational psychology courses are often taught by nonpsychologists will have no bearing on the matter since people almost universally assume that anyone who teaches a psychology course is a psychologist. If he is not a *professional psychologist*, the whole burden of representing psychology as a profession falls on the text.

In the second place, educational psychology is important to psychology as a proving ground for theory and as a source of new concepts. The educational psychologist who fulfills his function by actually working on educational problems is required to deal with problems which can be ignored or postponed in the laboratory. The need for the educational psychologist to deal with a wider range of phenomena has always tended to make him more receptive to new concepts and has generally caused theorists in educational psychology to anticipate the concepts of the experimental laboratories. To the extent that educational psychologists extend their services to education, we should expect that their contribution to basic psychological theory will have to increase in the same way that clinical psychologists, in assuming more respon-

sibility for clinical problems, have greatly increased their contributions to psychological theory. In fact, both of these areas are much more likely sources of fundamental theory than are experimental laboratories, which are best suited for testing it.

If this analysis is correct, anyone who wants to get a preview of future trends in psychological theory and, by way of the teaching profession, popular concepts of psychology and human nature, might do well to consider the content of current educational psychology tests.

Nature of the Individual

All seven texts focus their attention on the individual, who is regarded as a biological organism which is aware of itself and is actively, persistently, and more or less consciously seeking the satisfaction of certain basic needs, which are common to all people in all societies. Cronbach's statement (p. 100), that "Whenever a person acts, he is presumably trying to satisfy a need," is representative.

Behavior is molar and, in keeping with the general concept of activity from within, the term "stimulus" appears in the index of only one of the seven texts and is used there to refer the reader to a section critical of the S-R approach.

There is little disagreement about the nature of the basic needs, which are generally classified as physical (physiological) and psychological (in some versions, social or "socially derived"). Blair, Jones, and Simpson, along with Mursell, subdivide the psychological needs into social needs and ego needs. Cronbach points up the empirical nature of the specific needs cited by the different authors by denying the necessity of postulating a "need for security" by pointing out that "insecurity" is simply the

fear of failing to satisfy another need. There seems to be a tendency to pull back and avoid the confusion that would result from compiling a long list of competing "basic" needs. The multiple needs concept badly needs a principle for predicting which need will operate when a number of needs are unsatisfied. Commins and Fagin handle this problem by compressing all of the needs into one and treating them as aspects of one basic need for self-maintenance and enhancement.

Needs may or may not be conscious. They build up tension and make the organism sensitive to those aspects of the environment which promise possible satisfactions. "Stimuli function only in the presence of needs" (Mursell, p. 48). Goals are derived only to satisfy needs.

Educational Implications

The dynamic nature of the individual postulated by the needs concept implies that psychology has a role in determining the goals of education as well as in assisting in the improvement of methods. If the needs concept is correct, educational goals and practices not consonant with human needs are unfruitful and probably disruptive. The object of education, from this point of view, is the satisfaction of individual needs or, in a large social context, the promotion of mental health.

Heredity and Environment

The emphasis on basic human nature and on the body as a biological organism have brought about a withdrawal from the extreme environmentalist position held by many educational psychologists a few years ago. The physical structure of the organism is accepted as an obvious determinant of mental functions, thus providing a role for heredity as a determinant of behavior.

Learning

A trend toward a cognitive point of view has enabled the writers who have pursued it most to bring perception, learning, problem solving, remembering, and personality development together as a single process and has greatly increased the number of pages devoted to this "learning" process in their books. Blair, Jones, and Simpson devote 229 pages to learning, Commins and Fagin, 292, Cronbach, 183, and Mursell, 211. In contrast Remmers, Ryden, and Morgan give learning only 52 pages, Smith, 143 pages, and Sorenson, 88 pages, treating many of the topics discussed by the first four sets of authors under other headings than learning. All of the first four give versions of the learning process which stress perceptual reorganization and can be summarized as follows:

1. Learner's awareness of need or goal.
2. Existence of an obstacle.
3. This requires learner to interpret the situation.
4. Attack on the problem in terms of the learner's interpretation.
5. Consequences of learner's attack.
6. Reinterpretation of situation.
7. New attack.
8. Learning stops when learner is satisfied.

In this point of view both effect and frequency lose their place as independent causal factors. Since the learner stops reinterpreting the situation as soon as he is satisfied, no "reinforcement" is needed to stabilize the response. And, as Mursell points out, practice or repetition gives opportunities for reinterpretation but does not guarantee that it will take place. Even Sorenson, who approaches the problem from an eclectic point of view and makes frequency

the keystone of his learning system, seems to approach this when he remarks that repetition is not effective without interest.

The two other texts in the eclectically oriented group stress cognitive aspects of learning in their discussions without completely adopting the cognitive approach. Remmers, Ryden, and Morgan discuss trial and error, practice, and reasoning in terms of the learner's perceptions and accept insight. They explain conditioning by reinforcement. Smith, without citing Thorndike, uses exercise, effect, and belonging, and is the only author to make frequency or exercise an independent factor. He is also the only writer to modify the needs concept by accepting habit as a source of motivation. Commins and Fagin, and Mursell, who take the most consistently cognitive approaches, define habits as routine ways of dealing with situations which are formed, not by repetition, but by discovering what are for the time being satisfactory ways of response. These "habits" will be used as long as the corresponding situation is viewed in the same light and they result in satisfaction to the individual.

Transfer

One consequence of the rapid social and technical changes of the last few years has been the recognition that any education aimed at the teaching of specific facts and skills is apt to leave the student obsolete soon after he graduates. Awareness of the schools' need to educate their students for the "unforeseeable" (Cronbach) has revived interest in the problem of teaching for transfer of learning. The consensus of opinion is that transfer is best achieved by promoting understanding and making learning meaningful. "A concept be-

comes useful by being understood" (Mursell, p. 310).

In harmony with the cognitive-field approach implicit in most of the texts, the theory of transfer by identical elements is minimized by all authors except Smith, who does not discuss transfer. Remmers, Ryden, and Morgan and Cronbach retain them as one cause of transfer. Teachers are advised to help the student to form generalizations and productive attitudes, particularly toward the self, since self-attitudes affect behavior in all situations. Mursell points out that the student must want to transfer and Remmers, Ryden, and Morgan suggest that the student be taught to look for familiar landmarks.

Mental Health

All the texts accept the promotion of mental health as an important goal of education. There seems to be an increasing tendency to describe health and adjustment in terms of the personal perceptions of the individual, although social and behavioral criteria are not neglected. Commins and Fagin stress the need for stable perceptual organization; Cronbach, self-acceptance; Mursell includes a consciousness of respect by others. Remmers, Ryden, and Morgan state that the purpose of education is to help develop an individual "who is able to solve his problems successfully in his own viewpoint as well as in the view of others." The other three texts similarly say that mental health is achieved by the satisfaction of needs.

CONTRIBUTIONS FROM OTHER FIELDS

Physiology

One of the most striking things about these texts to readers who remember the frequent references to the

nervous system in early educational psychologies is the absence of such references in this group. The only index listing of the nervous system (in Mursell) leads the reader to an explanation of why consideration of what happens there does not help us understand learning. Two of the books discuss growth of the brain as an aspect of maturation but without going into neurological details.

Social Sciences

If we can judge by the indices, anthropology and sociology have contributed much more to these books than any other fields outside educational psychology itself. Havighurst is cited in as many books (five) as Guthrie, Hull, McGeech or Thorndike; and Allison Davis and Margaret Mead (four each) in as many as Gordon Allport or Carl Rogers. Only two psychologists, Hilgard (seven) and Lewin (six) are cited in more volumes.

Experimental Psychology

The point of view used in these texts owes much more to clinical and social psychology than to experimental or comparative. Having acquired a body of data in their own field, educational psychologists, if these authors are representative, have almost completely abandoned the old practice of extrapolating from laboratory data. In the hundreds of pages devoted to learning, B. F. Skinner is cited only three times, all in one text, and Spence is ignored by all. Even Tolman, very surprisingly in view of the theoretical climate of the texts, is referred to in only two of them. Similar neglect of the work of the Hanover Institute, Cantril, Harlow, and Tsai, all of whom have furnished abundant ammunition for the cognitive approach, indicates that the channels of communication be-

tween educational psychology and experimental psychology are not well travelled.

It may be objected that the small representation of experimental psychologists in these texts is due to the fact that they are written for teachers and not for psychologists. (Commins and Fagin seems to be the only one of the texts which is directed toward both.) It may be wise to keep a discussion of such sources at a minimum in the introductory, one semester courses. But the fact that the authors of so many outstanding texts have felt able to ignore the current work in experimental psychology may be a sign of trouble. This apparent rift between the traditional home of psychological theory and the field which we ordinarily think of as the major field for applying psychological theory may be a sign of healthy diversification or of impending dismemberment. In any case, the dissociation should not be allowed to become complete. And the primary responsibility for preventing further separation seems to belong to the experimental psychologists. As the situation now stands, the most popular problems for research in "pure" psychology are learning problems derived from an objective S-R approach. There is no doubt that this conceptual scheme offers the best opportunity for neat, nicely quantified experiments which are easy to plan and likely to give quantifiable results. The authors of these seven textbooks are, by their analysis of behavior, challenging this approach. Either explicitly or by implication, they are saying that the conceptual schemes that now hold the center of the stage in the laboratories are not, in their present stage of development, useful to them as educational psychologists, and that as a result they have had to develop and use others. In

comparison with the educational psychologist, who, like the physician, is bound by a professional obligation to do his best for individuals now, the experimental psychologist can work for more remote goals. At the present time he has two alternatives, either one of which is consistent with his obligations to society and to his profession. He can continue to refine the objective approach, in the hope that with further investigation and elaboration it will become useful in the applied fields. Or he can tackle the job of testing, refining, and clarifying some of the cognitive-field concepts that are being used in the applied fields. Since it would not be well to bet all our money on one horse, it is to be hoped that all experimentalists do not make the same choice.

Standards of Evaluation

In the reports on the individual texts which follow it is assumed that in addition to possessing the usual characteristics of any good textbook, a perfect textbook in educational psychology would be:

1. *Systematic.* The conceptual scheme should be adequate to make the subject and the student's own observations meaningful.
2. *Related to education.* The implications of theory for educational practice must be explicit.
3. *Related to psychology.* Educational psychology should be treated as an aspect of psychology and add to the student's knowledge of psychology as well as of education.
4. *Well documented.* Both psychology and education should be presented as fields for research. Theories should be presented as theories, not as revealed truth.
5. *Self-contained.* It is not possible to assume that the student using the book will have had any previous courses in psychology.

It is obvious that each of these characteristics conflicts with others. A book focused directly on educational problems is likely to lack systematic coherence. A book which concentrates on the psychological background is apt to lose touch with the interests and needs of school teachers. In addition, educational psychology is taught at almost all levels of college and graduate school, and for either one or two semesters. A two-semester book which meets our criteria would be so highly integrated that it could not be cut down for a satisfactory one semester course by omitting chapters.

BLAIR, JONES, AND SIMPSON

This book is closely tied to classroom problems. The theoretical position is consistent without being extreme or obtrusive. The authors have succeeded in striking a judicious balance without confusing immature readers with too many pros and cons. Taking the group of texts here reviewed, this is the "typical" book. Because of its clear presentation and reading ease it is a particularly good prospect for one semester courses. The chapters on evaluation of the school and on the teacher as a person are particularly good.

COMMINS AND FAGIN

The cognitive-field approach which is implicit in the majority of books here reviewed is spelled out in detail by Commins and Fagin. This book is written for psychologists as well as for teachers. I have the feeling that this forthright and sophisticated volume is primarily a psychology text in which the authors take over the field of education just to demonstrate the effectiveness of their point of view. Anyone who wants to relate the popular needs theory to the main stream of psychological theory would

do well to read this book. Their discussion of learning from the cognitive-field point of view is the best to date. In all of our criteria except relation to educational problems this book is very high. Its relative weakness in that area is a result of the same orientation toward psychology which makes it an important contribution to educational theory. It seems best suited for students who are experienced teachers.

CRONBACH

Cronbach's text has caused more people to give up their plans to write an educational psychology text than any book that has appeared in years. It is unobtrusively but consistently systematic, well documented, and closely tied to educational problems which are presented at both the philosophical and classroom levels. The book itself matches the text. There are 97 illustrations.

The contrast between Commins and Fagin and Cronbach is an interesting one. Point by point, the theoretical approach of the two books is identical. But while Commins and Fagin use it in a whole-hearted attempt to feed it back into psychology and conquer new fields there, Cronbach is so uninterested in his conceptual framework, except as a convenient way of dealing with educational problems, that his text is completely inoffensive to advocates of other points of view. He is attempting to sell a point of view in education but not in psychology. In large departments where the educational psychology course is taught by several people with differing points of view about psychology, this is an important advantage for Cronbach.

This book rates near the top in all criteria and has already become a standard of comparison for other educational psychologies. It does

have defects. The many case studies are apt to be confused with one another. It is just too big and too thorough for a one semester introductory course, although upper classmen who have had introductory courses in human development can manage it in that time.

MURSELL

Mursell stands with Commins and Fagin as an uncompromising, all-out exponent of the cognitive-field approach. The difference is that, unlike them, he is primarily interested in education. This is a brilliant and original book, closely reasoned and well documented with few references from outside the field of education. As a result it is not likely to give its readers much information about other points of view and is not intended to do so. Mursell's insights are acute and the book deserves thorough reading by psychologists. The presentation is so clear and well organized that it is a possible book for a one semester course at the undergraduate level.

REMMERS, RYDEN, AND MORGAN

Of the seven books, this book is the one farthest removed from the systematic position of Commins and Fagin and of Mursell. The text is characterized by a minimum of theorizing. It stresses environmental

causes of behavior more than other books in the group. Although the needs concept is used, several statements seem to imply that the individual is stirred to action only by environmental stress. One result of this point of view is an excellent summary of social class as it related to education. Another strong point is an unusually long and good section on tests, interpretation of scores, and measures of correlation.

SMITH

This is a competent and somewhat conventional text at the undergraduate level. Smith gives the student a little of everything, stirring up needs theory and habit in the same pot, but he presents it charmingly and at a low level of reading difficulty. Departments offering one semester lower division courses might well consider his text.

SORENSEN

Sorenson's well-known text, now in its third edition, has the advantages and disadvantages of the other texts which do not have a tightly defined, systematic point of view. Its strong points are a comprehensive selection of materials, a very readable style, good illustrations and summaries, and a legible format. It has been one of the most popular texts and will continue to be.

BOOK REVIEWS

STONE, CALVIN P. & McNEMAR, QUINN (Eds.) *Annual review of psychology* Vols. 5 & 6. Stanford, Calif.: Annual Reviews, Inc. 1954 & 1955. Pp. lx+448 & vi+517. \$7.00 (each Vol).

A reviewer setting out to review the *Annual Review* feels himself caught in the toils of an infinite regress. One way of escape lies in an over-all appraisal, rather than the more usual chapter by chapter treatment, but Lanier has already done this admirably for the first four volumes.¹ Perhaps the fact that the sample has now increased by 50 per cent may serve as an excuse for reopening some of the more general questions.

Lanier quotes from the preface to Volume 1 where it is stated that the authors were asked to "adopt an interpretative and evaluative approach to the literature they selected for review." He felt that in this respect Volumes 3 and 4 improved upon their predecessors, but still contained a number of chapters which were merely "bibliographic digests." An attempt was made in the present review to assess Volumes 5 and 6 using the same standards. Volume 5 seemed to regress slightly but Volume 6 was in this respect the best so far. Even here, however, evaluation and interpretation were not always conspicuous.

Instead of blaming the authors it may be well to ask whether an "interpretative and evaluative approach" is possible on an annual basis. Such an approach would seem to involve the drawing out of implications from experimental data, the

restructuring of problems in the light of evidence from different areas, and above all the identification of trends. These things are not easy when time presses, and the last of them can hardly be done at all. Psychology, like the watched kettle of the proverb, changes so imperceptibly under this constant scrutiny that from one year to another it may not seem to change at all. The identification of trends is more appropriate to a quinquennial than to an annual publication.

Then again many readers may want a competent bibliographic digest. The research worker can go to *Psychological Abstracts*, but the busy teacher or clinician must often find in the *Annual Review* an invaluable clue to the labyrinth of the journal literature. So long as the material is complete and objectively presented, he is quite prepared to do his own evaluation.

Looking at the two volumes under discussion with this thought in mind some qualification appears to be necessary. Where the topic itself provides the organization, as for instance in the chapters on the special senses, the factual approach works admirably, and the relevance of the data presented is established by a fairly well-articulated context. Elsewhere, however, this may not be so, and a merely bibliographic treatment sometimes leaves lost little facts adrift on pages. For example, "In a sharing test the only child was more selfish, on the average, than the child with sibs, according to Ugurel-Semin."

Particular chapters, again, seem to justify the original terms of reference quoted above. For example, E. Lowell Kelly on "Theory and Techniques

¹ Lanier, L. H. An evaluation of the *Annual Review of Psychology* (Vol. I-IV) *Psychol. Bull.*, 1954, 51, 180-190.

of Assessment" in 1954 and MacCorquodale on "Learning" in 1955 have written penetrating and objective surveys in which the interpretation and evaluation is of the highest value. On the other hand it must be kept in mind that although these chapters reported the literature in their fields and periods they were really much wider in their scope. Each contained a far-reaching appraisal of the present situation as well as sound counsel for the future. It will be some years before the same thing can be done again, and probably much longer before it can be done as well.

Other chapters justify their approach in other ways. Tolman and Postman's provocative discussion of papers on learning in the 1954 volume gains from its avowed bias, though here and there the writing is a little slapdash. "It would seem to the reviewers that nature must be even cleverer about providing the right environmental stimuli for combining movements into adjusted, directed acts than we would have supposed her capable of." Here and elsewhere the authors might have done a little more running back and forth in front of what they had written, but a coldly objective treatment, however grammatical, would lose by comparison with the present chapter. Another idiosyncratic chapter in the same volume is by George Miller on communication. He selected his papers "by rigid adherence to a single rule: Only those studies were covered that interested the reviewer." On this basis he has put together a lively, forward-looking discussion which might have been impossible had his bibliographic burdens rested more heavily upon his shoulders. On the other hand the danger of allowing the reviewer too free a hand, particularly in a central area, shows up in

the 1955 chapter by Kallmann and Baroff on "Abnormalities of Behavior." Here the original title has had to be supplemented by "In the light of psychogenetic studies" presumably in order that it may represent more nearly what is in fact discussed.

The upshot of all this seems to be that it is possible to justify a sound objective survey of a year's research, particularly in the more structured areas, and it is possible to justify an "interpretative and evaluative approach." But some question remains as to the best way in which the two can be combined. Assuming that interpretation and evaluation can hardly be made afresh in each year, although they remain an essential element in the series as a whole, it might be worth while to envisage some more formal provision for them at longer intervals. A sort of rotation could no doubt be set up among the various fields which would permit each to lie fallow so far as the more systematic kind of treatment is concerned. In any given volume the authors charged with interpretation and evaluation might be allowed the time and space their task requires. The idiosyncratic chapters might be left to happen as happen they will.

Volume 6. In the concluding paragraph of their preface to this volume, the editors say: "When scientific psychology flourishes on a world-wide basis, as in a few nations of the western hemisphere it has developed during the past half century, the *Annual Review of Psychology* will fully realize its major aims: to disseminate the fruits of research in psychology and to promote collaboration among scholars of all nations. . . ." Leaving world wide plans to the future, it seems reasonable to ask how well the *Annual Review* is serving the "few nations of the

western hemisphere" now. A study of the literature cited reveals some interesting differences between one area and another. Classifying the sources into "American" and "Foreign" we get the following figures:

	American	Foreign
Child Psychology	101	2
Learning	148	0
Vision	159	35
Hearing	117	23
Somesthesis and Chemical Senses	29 (36)	37 (18)
Individual Differences	75	3
Personality	58 (49)	26 (0)
Social Psychology	137	2
Industrial Psychology	116	6
Comparative Psychology	59	32
Physiological Psychology	119	31
Abnormalities (Psychogenetics)	96	48
Assessment	155	8
Psychotherapy	89	4
Counseling	78	0
Statistical Theory	84	20
Educational Psychology	109	3
Problem Solving and Thinking	161	4

Since the authors of the chapters on "Somesthesis and the Chemical Senses" and "Personality" were foreign, the corresponding figures from 1954 have been added to make inter-area comparison easier. It would seem that the sensory, physiological, genetic and statistical chapters resemble one another in having a considerably greater proportion of foreign references than do those with a more narrowly psychological content. Classifying along these lines and using the 1954 figures for the two chapters mentioned above, we get a mean foreign percentage of 34.2 for the first group of chapters and 2.73 for the second. The difference is significant, but what does it signify? There seems no reason to suppose that one group has more linguistic versatility than the other; in fact the chapters by Kallmann and Baroff and by Teuber have fewer foreign references than the average

for the chapters with which they have been classified. The suggestion that there is less European work going on in the areas with low foreign reference scores seems plausible for learning theory and counseling which are to some extent American preoccupations, but this answer does not appear to be altogether adequate elsewhere. Nuttin found 26 European studies on Personality worth reporting in 1955 whereas Child in 1954 had found none. Similarly Heron's 1954 chapter on Industrial Psychology lists 33 European titles as compared with 6, all from one journal, included by Wallace and Weitz. Undoubtedly more work is going on in America than elsewhere, and a proportion of three to one seems reasonable. A proportion of thirty to one suggests that in many areas research reported overseas is being neglected. The significant difference referred to above is probably due to the fact that some areas are more highly integrated than others, and what is discovered anywhere is relevant everywhere. As in all established sciences a parochial attitude is incompatible with progress. In other areas data tend to accumulate piecemeal, and accessibility alone determines what shall be added to the heap.

On the other hand it is clear that, if the *Annual Review* is going to realize the aims laid down in the Preface to Vol. 6, some further thought must be given to this problem. Though to the reviewer difficulties of time and language seem insuperable, the impressive performance of some authors suggests that the level of aspiration might be raised for all.

Looking back over this review it seems that too much time has been spent trying to tell a body of able and energetic psychologists how to

do better what they are already doing extremely well. The *Annual Review* fulfills an indispensable function in the psychological world, and the 1955 volume worthily continues a short but honorable tradition.

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ALLPORT, FLOYD H. *Theories of perception and the concept of structure*. New York: Wiley, 1955. Pp. xxii + 709. \$8.00.

This book is in reality two books. The author planned it as one: the earlier chapters read as if preparatory for the later, as the later chapters purport to be consequences of the earlier. But the reader is altogether likely to find it the product of two basically different intellectual enterprises, which are well announced in the double title. The first twenty chapters give us a critical survey of many theories of perception while the last chapter presents the author's very original concept of "structure" which may be applied to, but does not necessarily arise out of, discussions of perception.

I. As a review of the many theories suggested by experimental research on perception, Allport's work is noteworthy. He has certainly done a conscientious job on each of the thirteen major theories—ranging from the Titchenerian core-and-context, to the cybernetics of Wiener and of Pitts and McCulloch, and including such variety as the Gestalt, the Hebb cell-assembly, Helson adaptation-level, Brunswik probabilistic, and the Bruner *et al.* directive-state. As each in its turn is taken up, the author is at great pains indeed to give it an adequate and detailed exposition—even occasionally at the expense of considerable restatement and/or meticulous and finespun argumentation. Each is a model of clarity; and many

presentations of a technical system of thought with scarcely any diagram at all is an object-lesson in effective and precise and yet graceful use of the English language. It is, in fact, something of an intellectual treat to follow Allport, especially as his thoughts weave in and out and sweep backward and forward in utter command of the so diverse and often recondite theoretical systems. Incidentally, he does not burden the pages with pedantic chapter and page references; and less than three hundred works are cited in the course of the whole survey.

After exposition, criticism! Each field of experimental research and the theoretical system arising from it is examined and appraised; and where each falls short of meeting the complete demands of a perceptual theory is carefully (sometimes repetitively) pointed out. The reader frequently feels that he is an auditor at an Allport seminar on the Psychology of Perception. And he may recognize some of Allport's ways of thinking: his emphasis upon the close inductive relation between experimental finding and theoretical superstructure; his lifelong hardheadedness toward the enthusiasts for molar (and institutional) explanation who forget that, after all, without the parts you have no whole; his meticulous thoroughness in canvassing the implications of a viewpoint; his insistence upon the parsimonious explanation; his respect for logical consistency.

Toward the end of this survey the reader comes upon a baffling Chapter Nineteen in which is pointed up one defect in all the theories: "the unsolved problem of meaning." The present reader at least was led to anticipate a positive contribution to supply this lack. On the contrary he found himself in agreement with one of the authors cited (Pickford)

in whose article he found this sentence: "The idea of meaning is a psychological abstraction, and it is necessary to guard against the risk of treating it as if it were a discrete entity."

Nevertheless, the present reviewer is of the opinion that for anyone about to work in the field of perception, this book will be a first recommendation, both for inclusiveness of the perspective and for lucidity of each portrayal.

II. Following this exhaustive survey of the thirteen major theories of perception Allport does not attempt to reduce them to one; instead he draws up some eight basic (and quite abstract) generalizations. The perceptual process has, in all the theories, had the character of internal relatedness, circularity or self-closedness, space and time building, flexibility or deformability, persistence of constant relationships, level-maintenance or equilibrizing, weighting and pooling of energies or dimensions, and interaggregate facilitation or opposition. These broad (and formal) conceptions give us our most substantial insight into the nature of perceptual process, over and beyond the basic fact that a perceptual act is really a dynamically operating structure.

Prior to and independently of his examination of the theories of perception Allport had been seeking generalization of all natural laws in the concept of "structure," as set over against number or dimension or quantitative description, not a spatial or a temporal structuring simply but an event-structuring, a pattern of "ongoings" and of "events." Ongoings include such things as neural impulses or muscular contractions (to employ only neural illustrations here). Events are "junction points" as when an ongoing stimulus-process

hits a receptor or an ongoing neural process at a synapse fires a succeeding neuron. The enterprise the author is embarking on is what may properly be called a metaphysics—in the original and precise use of that term as connoting the effort to see the world in its most abstract and generalized aspects, with no implication at all of a sneaking behind the scenes but simply that of highest-level description.

To give a touch of concreteness to his highly abstract theory of event-structures, Allport suggests a simple mechanical model or analogue, a visualized schema. "Let us think of a thin wire hoop, capable of being bent in any direction or shape, but always remaining a hoop, upon which are placed, at intervals, a number of cross-marks . . . cutting the hoop into segments. The model is, of course, kinetic, not static. The segments of wire into which the hoop is broken by the cross-marks represent ongoing processes; the cross-marks are the events between these ongoings . . . 'event-points.' A succession of ongoings and events thus occurs around the hoop, and the succession can repeat itself indefinitely" (p. 634). With the one hoop we can conceptualize a complete and repetitive cycle of ongoings and events. With two such hoops at right angles to each other we can visualize an event-system. With a number of hoops put adjacent in a larger circle of hoops, each touching two others at event-points, we have a model for a hierarchy, or cycle of cycles of ongoings and events. The present reviewer will not follow the further elaborations of this exposition of key-concepts illustrated with this hoop model; the reader can do so by consulting the book. Suffice it to say that manipulations of the hoops furnish the author with visualized par-

allels for observed facts of organismic behavior, as well as for particular principles of configuration of learning, of equipotentiality and mass action, of equilibrium and steady-state, of set, of motivation, of constancy, of facilitation, and so on.

Unquestionably there is ingenuity manifested throughout all of this. Yet the present reader has experienced the repeated qualm that it is all highly artificial, that the architect of the hoops has devised them to schematize certain principles already observed, without obtaining any new insights into the phenomena themselves. If there is benefit to be derived from such purely analogical and artificial parallelisms we may expect to find it in any new hints and suggestions given to research workers. (After all, the farfetched diagramming of Lewin did lead to new envisagements of concrete research problems. And other cases are matters of history.)

Returning now from diagrams to principles, the question remains: how much will Allport's concept of structure clarify the problems of perception? This reviewer would leave it at this: in the same measure that his structural theory serves as a general integrating law for nature, and more particularly to the extent that the notion of "ongoings" and "events" turn out to prove fruitful to those who work on perceptual problems.

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Révész, G., (Ed.) *Thinking and speaking: A symposium*. Amsterdam: North-Holland Publishing Co., 1954. Pp. 206. \$4.00.

This book is a reprint of a double number of *Acta Psychologica* and contains ten contributions. Five are by psychologists (Eliasberg of New York, Kainz of Vienna, Piaget, Cohen

of Manchester, and Révész), one is by the philosopher Jørgensen of Copenhagen, one by the mathematician Waerden of Zurich, two by psychiatrists (Goldstein of New York and Gruhle of Bonn), and one by the linguist Buyssens of Brussels. English summaries are provided for the six articles not in English. Révész states the problems: Is speech possible without thought? Is thought possible without speech? He answers the first question affirmatively. The second is more difficult, but he reaches the conclusion, after examining many kinds of thought, that thoughts that are *wortlos* are not *sprachlos*, but are without exception based on the language-function. The reason for this conclusion is that wordless thoughts during their development have a syntactical structure, an anticipated schema in Selz's meaning of these terms. Révész also emphasizes the importance for thinking of memories of events previously formulated in speech. He believes that he has proved that the two functions are one and indivisible (though speech may occur without thought). Piaget in a genetic study states that the two functions are "supported by each other and are in a perpetual interaction, but either depends on intelligence, which is anterior to and independent of speech." Cohen's paper is a thesaurus of opinions of past writers on these problems and also presents a preliminary experiment on the effect of delayed recording on the quality of thought, in which students were required to write imaginative stories on a set theme, starting to write either immediately or after a period of time in which they thought about the task. The other papers offer a great variety of detail about animal speech, aphasia, abstraction, the thinking of mathematicians, and language in re-

lation to Goldstein's concept of self-realization. In an epilogue Révész says that these distinguished authorities have not come to an agreement, but their efforts have led to important conclusions and have cleared up many misunderstandings and paradoxes.

This book should be read by all American psychologists who are interested in keeping abreast of the developments of *Denkpsychologie*. It seems somewhat foreign to the atmosphere of American psychology, with its preoccupation with newer problems than this one which seem to offer more hope of solution by other means than discussions of this kind, however excellent they may be.

FRANK A. PATTIE

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Handbook of human engineering data.

Port Washington, N. Y.: Special Devices Center, Office of Naval Research, 1952. (2nd Ed. Rev.) Pp. 607. \$9.00.

This *Handbook* was developed by the Institute for Applied Experimental Psychology of Tufts College, Medford, Massachusetts, under contract with the Special Devices Center of the Office of Naval Research. Work on it was begun in 1947. After three years of work by the Tufts Handbook Staff and a large number of collaborators over the country, the first edition was issued in loose-leaf form in December, 1949, not as a finished product, but rather as a step in achieving a more complete reference work. This was the first major publication of its kind to appear in the newly constituted field of human engineering, and like any pioneering effort, it faced many difficult problems of the type which usually require a stepwise solution.

The Second Edition of this *Handbook*, issued in August, 1951, was a

combination of the original plus Supplement No. 1. Supplement No. 1 included an entirely new Part IX, *Learning*, and a section on *Altitude* fitting into the already existent Part VII, *Physiological Conditions as Determinants of Efficiency*.

The Second Edition (Revised) differs from the Second Edition in the replacement of the original Part VI, *Motor Responses* with a completely rewritten Part VI, *Motor Performance*, including the associated indexes and glossary, which is known as Supplement No. 2. The essential features and plan of the original edition have been adhered to throughout all of the releases. This includes a 22-page glossary and an author and subject index at the end.

Two major problems which a reference work of this type faces are the intended audience, and the rationale of organization and presentation of materials. Who is going to use the book, how, for what, how frequently and under what circumstances? Now these are obviously not new problems; they are simply of special significance because of the nature of human engineering information and design engineers, who were originally conceived as the audience for the *Handbook*. A fine job of literature reviewing and of condensation of information about human beings has been done by the Tufts College staff. It turns out that the manner and method of presentation and organization is not so felicitous from the point of view of the intended engineer users. This is not entirely true because there is a small percentage of engineers who have been aware long enough of the fact that they must consider "user" characteristics if their design work is to be successful, and have done something about it, to have developed some understanding of pertinent data. The great majority, however,

appear to need "answers" initially without personal study of an additional specialty (to them). The sophistication required for effective reading of the *Handbook* and for ready access to desired subject matter therefore renders it less useful to the large majority of engineers than is to be desired. However, by way of compensation, the *Handbook* has been found to be an effective reference by workers in several other fields such as engineering psychology and other phases of human engineering, military personnel, training activities, and business establishments, to mention only a few.

To many nonpsychologists the *Handbook* provides the means whereby they first become aware of the broad panorama of scientific concern with man in a nonclinical sense. This reviewer also believes there should be a growing place for the *Handbook* in training programs throughout the country, whether in college, government, or industry.

Much might be said about the problems of display of information and layout of the book, but particular solutions to these problems are so intimately associated with content and intent that perhaps the only things worth saying in a review are that (a) there is more emphasis on giving information on human beings than on recommending engineering practices which indicate effective use of the data, (b) the organization is based upon human topics, not engineering ones, so that engineers report having difficulty finding the data relevant to their particular problems. Be that as it may, it is hardly to be expected that a single reference handbook could be prepared that would meet all of the needs of design engineers and of the motley field of human engineering at one fell swoop! The book already meets important

needs and offers promise of meeting others as already indicated above. This will take place as more nonpsychologists become aware of the need for considering human factors systematically in making things for people to use or operate, and in modifying the environment in which men must work or live.

The *Handbook of Human Engineering Data* in the opinion of this reviewer is an important contribution which, teamed up with two other major publications in human engineering—Chapanis, Garner, and Morgan's *Applied Experimental Psychology* (Wiley, 1949) and Woodson's *Human Engineering Guide for Equipment Designers* (University of Calif. Press, 1954)—provides a strong basis for growth and effective practice in human engineering. Consequently people interested therein should have the *Handbook* available to them.

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McNEMAR, QUINN. *Psychological statistics*. (2nd Ed.) New York: Wiley, 1955. Pp. vii+408. \$6.00.

This edition maintains the useful pedagogical device of stressing algebraic derivation as an aid to understanding of formulas. Teachers and students who liked the content and style of the first edition should like the new edition even better since it is an improved arrangement and elaboration of the 1949 version.

Because the original edition is so well known it suffices to note some of the topics which have been clarified or added. Much fuller discussion is provided of types of error and power considerations in hypothesis testing as well as the problem of designating one-tailed vs. two-tailed critical regions. Among the "significance tests" which were not presented in

the first edition are chi square for several correlated proportions, differences for related variances, differences for related correlations, Bartlett's test for homogeneity of independent variances, exact probability for the 2×2 table, and a reference to the comparison of means when variances are unequal. Brief attention is also devoted to intraclass correlation, the discriminant function and Kendall's tau while a more friendly attitude is taken toward the point biserial and the phi coefficient.

The most extensive revision has been made in the treatment of analysis of variance, which presents an excellent discussion of underlying mathematical models and their relation to choice of error term for F tests. In a new section on latin square, McNemar offers a strong argument for limiting use of this design in psychological research, but does not mention the experimental advantages of replicated latin squares. In view of recent trends and his conservative attitude toward Type 1 errors, it is of interest that McNemar still advocates use of the t test for comparing individual means following a significant over-all F . Since a section is devoted to the handling of unequal frequencies in the single classification design, it would have been appropriate if reference had also been made to the common problem of disproportionate subclass frequencies in multiple classification design.

A new chapter on nonparametric methods contains four pages. As McNemar notes in the preface, "Some will be critical of Chapter 18 because it does not contain all the several so-called nonparametric techniques."

It is superfluous to add that this text is a very solid introduction to descriptive and inferential statistics. The reviewer understands that the

first edition was widely adopted. Research will continue to benefit as the new edition is mastered by students of psychology.

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PIAGET, JEAN. *The construction of reality in the child*. Translated by Margaret Cook, New York: Basic Books, 1954. Pp. xiii+386. \$6.00.

The way in which the infant organizes the universe of objects in space is analyzed from observations and experiments made by Piaget on his son and two daughters from birth to 18 months. To elicit responses, objects were moved, dropped, hidden behind screens, partially exposed, moved so that part of the trajectory was hidden, etc.

In the first two stages (up to 6 months), the infant's universe is not cut up into objects but is like a stream of pictures without substantial permanence or spatial organization. But every intersensory coordination, every movement, and every functional use of sucking, sight, hearing, and touch contribute to the anticipations which assure the solidity and coherence of the external world. If the new baby had object-concepts, he would search for vanished objects and remove obstacles. But he does not conceive the vanished image as an object which exists in space and remains identical with itself.

In the third stage, if the object is not found within the exact extension of his movement, the child gives up hope of finding it. When prehension comes, the child learns to follow with his hand objects which escape him and thus begins to attribute permanence to tactile objects.

In the fourth stage, the child searches with his hands for the dis-

appearing object without reference to the displacement. When he glimpses any part of the object he shows greater desire to see it than when he catches sight of the whole. He is trying less to free an object masked by a screen than to free his own perception.

In the fifth stage (13 to 18 months) permanence is attributed, since the child not only expects to find vanished objects in the places where they were left but also in the extensions of their trajectories.

In the sixth stage, the child can construct invisible objects by means of mobile schemata which can be combined in various ways. Thus he discovers the object's real permanence and constructs a collective universe.

The development of the spatial field begins in the first two stages with heterogeneous and practical groups in which each perceptual bundle constitutes a space. In the third stage practical groups are coordinated into subjective groups, while in the fourth stage reversible operations are discovered and a transition made from subjective to "objective" groups. This becomes complete in the fifth stage, which makes possible a sixth stage in which the child can, by the representation of movements, rediscover a hidden object after several sequential displacements, even if some occur outside the visual field.

Similarly during the first two stages of the development of causality, the child makes contact between internal activity and the external environment by means of pure reflexes and elementary habits. The third stage, that of magico-phenomenalistic causality, begins with the coordination of prehension and sight into secondary circular reactions. Elementary permanence is attributed

to things as a function of the action, and systematic interest in causal relations appears. For example, the child who sees his hands and feet move in the visual field and who already controls movements of his hands to some degree, comes to their causal connection which is so clear to the observer but not to the child. In the fourth stage the child begins to externalize and objectify causality; in the fifth stage real objectivation and spatialization become evident. In the sixth the universe has come to be an independent system of causes and effects, among which the sequences of his own acts are placed as elements in a totality which transcends them.

In general the organization of the temporal field parallels that of space and complements that of objects and causality. In the first two stages there are practical time relations, since the child knows how to coordinate his movements in time and to perform certain acts in a regular order. But he is unconscious of the unfolding. In the third stage the child begins to act upon things and to use their interrelations through prehension of objects. Because these movements depend upon the personal action of the child they are called subjective. Secondary circular reactions appear which in the fourth stage result in schemata which make possible application of familiar means to new situations, and thus lead to the objective series of the fifth stage in which time is applied to things and a continuous and systematic linkage formed which unites the events of the external world to one another. In the sixth stage the child goes beyond the present by evoking images or language signs which constitute the representative series.

Three formative processes are necessary to the elaboration of the object concept; the accommodation of

organs which makes possible the foreseeing of the appearance of bodies; the coordination of schemata which makes possible the endowment of each of these bodies with many interconnected qualities; and a form of sensorimotor deduction which makes possible the understanding of the displacements of bodies and the reconciliation of their permanence with their apparent variations. These functional factors—foresight, coordination, and deduction—change entirely in structure when they pass from the sensorimotor plane to that of speech and conceptual operations, when systems of classes and thoughtful relations are substituted for simple practical schemata.

Thus in his first eighteen months the child proceeds from an initial practical solipsism to the construction of a universe in which he is an element in a stable world. In the initial state the universe is neither substantial nor extended in depth as it is related to a subject ignorant of himself and perceiving reality only through his own activity. The final stage is that of a solid and vast world obeying the laws of conservation (objects) and kinematic ones (groups) in which the subject places himself consciously as an element. From egocentrism to objective relativism seems to be the formula of this law of evolution.

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ROGERS, CARL R. & DYMOND, ROSALIND F. (Eds.) *Psychotherapy and personality change*. Chicago: Univ. of Chicago Press, 1954. Pp. viii+447. \$6.00.

This is a truly impressive book, if one holds any brief for the value of objective research where psychotherapy is concerned. It is still much too early to hope that discussions of

psychotherapy can be aimed at concrete problems involving specific principles. Practical reality still demands the teaching of untried principles spiced with a bit of emotion to help convince the apprentice that he is getting his money's worth. This progress report from the Counseling Center of the University of Chicago should, however, convince anyone that objective research on psychotherapy is possible and may, eventually, pay off with concrete results.

It should also convince the faint-hearted that research on psychotherapy with real patients who have real problems might best be left to those who have great courage, considerable dedication, and the foresight to equip themselves in advance with a large grant from a foundation. This kind of programmatic research in psychotherapy cannot substitute for the brilliant hunch, but the brilliant hunch must inevitably be subjected to the torture of programmatic research.

Twelve members of the staff of the Counseling Center report separate but interrelated studies of the same group of twenty-nine unselected clients who received client-centered psychotherapy. As the studies were designed to evaluate the outcomes of this particular brand of treatment, two simple but, from the practical standpoint, quite difficult control procedures were devised. Half of the clients receiving therapy were asked to wait sixty days before starting treatment in order to determine if expressed desire for treatment produced changes in adjustment. The results were negative. The second control consisted of a matched group of persons who had volunteered to participate in research on personality. Although the second control group did not receive treatment, they were subjected to the same tests as the

therapy group in order to determine whether time produces changes in personality. The results again were negative.

The simplicity of such a design belies, of course, the efforts necessary to bring to fruition a four-year study of eighty subjects and sixteen therapists. The measuring instruments, moreover, present the same problems of validity and reliability in a carefully worked-out research design as they do in the diagnostic clinic. In line with Rogers' interest in the self concept as an explanatory construct in personality change, major interest centered on Q sorts. In addition, the Thematic Apperception Test, the Willoughby Emotional Maturity Scale, attitude scales, therapist ratings, friends' ratings, a situational test, and phonographic recordings of all treatment interviews, comprised the data for the objective studies. The six-hour battery of tests was administered to clients and controls twice before treatment of the clients was begun, again at the conclusion of therapy, and, finally, six months to a year following the end of treatment. The Q sorts (used to compare self, self ideal, and the ordinary person) were administered several times during therapy depending upon the length of treatment. Two follow-up interviews and a follow-up questionnaire completed the pattern. All analyses were based upon carefully worked-out hypotheses which were congruent with some aspect of personality theory as viewed by the Chicago group.

The results are fairly clear-cut in showing that the therapy group underwent significant changes in their attitudes toward themselves as measured by Q sorts. These changes were not found in the control groups. Furthermore, the post-theory concepts of self in the therapy group approached

more closely the clients' self-ideals and their conceptions of "other" people. The latter two concepts remained relatively constant. During the follow-up period, regressive changes took place in some of the therapy group whereas other members held their gains.

The results of the Q sorts were, in general, upheld by analyses of the other measures utilized. Of particular interest, because of its novelty, is the analysis of friends' ratings of behavior. The friends were kept in ignorance of the therapy. For clients who were judged to be making progress in therapy, the friends' ratings indicated a definite increase in maturity of behavior which was not found in friends' ratings of clients who were not improving. For once, therapy research combined measures of self-evaluation with external ratings of behavior in *normal* social situations.

With the more intensive analysis of the phenomenal field which this book represents, the expected perplexing problems arise. A positive relationship seems to exist between *negative* self-evaluation and internal tension. However, where *positive* self-evaluations are concerned, the interpretation is not so clear, as the positive statements may be valid or they may represent defensiveness. Such a possibility, of course, raises considerable question about the validity of any method which depends on positive self-evaluations as indicators of change in personality integration. The dilemma may not be insoluble.

This book is another testimonial to the fact that psychotherapy is rapidly becoming a legitimate field of scientific research as well as an applied art.

VICTOR RAIMY

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GRINGS, WILLIAM W. *Laboratory instrumentation in psychology*. Palo Alto, Calif.: The National Press, 1954. Pp. vi+282. \$4.75 (cloth), \$4.00 (paper).

In many areas of psychological investigation, it is obvious that progress is dependent upon developments in technological fields far removed from the introspectionist's armchair. It is equally clear that the meaning of data gathered with the help of any gadget more complex than a pencil is relative to the instrumentation. Appreciation of the possibilities of use and misuse is a prerequisite for both competent investigation and critical evaluation. And so the techniques course has become an increasingly familiar component of graduate training, and psychologists of older vintage often find themselves in need of informal tutoring.

It has been Grings's purpose to provide in convenient form a discussion of the basic characteristics of representative stimulating and recording systems, principally for use with human subjects. The material is organized in seven major chapters—Behavior Recording Systems, Timing and Counting, Audition, Vision, Other Senses, Human Learning and Perception, and Bioelectricity—and is presented in varying patterns. In some cases there is not only description of the apparatus itself but also a fairly detailed exposition of the experimenter's manipulations: how to smoke kymograph paper and fix the record, how the oscillograph beam can be shifted on the tube face, how scalp electrodes are applied and removed, how the visual field is mapped in perimetry. In other instances (e.g., in the consideration of timing devices), it seems more appropriate simply to describe the equipment, perhaps with the aid of a wiring diagram.

It is explicitly stated that the book is intended to be an introduction rather than an ultimate guide to research. The primary effort is to suggest by illustration the types of question that must be asked when apparatus is adopted to extend measurement and control. It is abundantly clear that the practical choice among procedural alternatives is specific to the requirements of the particular experimental situation, often in the nature of a compromise that is constantly under review as additional technical refinements become available.

The text is liberally sprinkled with line drawings and cuts, with an average of one or more every two pages. Many are helpful, a few are not at all illuminating (some exterior views of commercial items are prime examples in this category), several are confusing because of inconsistent or incomplete legends, and one or two are misleading. There is some unevenness in the inclusion of references for advanced study, but the author offers a mimeographed bibliography of more than 1,000 relevant items. Altogether, the usefulness of the book far outweighs its limitations, and the advanced student or the teacher in laboratory courses will find it of considerable help in surveying the technical tools of the trade.

FRANK W. FINGER

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BECK, SAMUEL J., *The six schizophrenias: reaction patterns in children and adults*. New York: American Orthopsychiatric Association, Inc., Research Monograph No. 6, 1954. Pp. viii+238. \$5.00.

A coordinated inquiry into problems of schizophrenia, as evaluated and dealt with by psychiatric method, psychological method, and social work method, is the aim of this

book. In a study of sixty children and fifty adults, the author attempted an interdisciplinary approach, built mainly around diagnostic Rorschach test evaluations made by psychologists and clinical behavioral evaluations made by psychiatrists, thus leading to a correlative approach between psychiatric findings and Rorschach test results.

The outline of clinical psychiatric formulations and their Rorschach correlates includes such areas as (a) the defenses, as assessed through obsessive-compulsive behavior, withdrawal from stimuli, projection, rigid hold on self and on reality, metaphysical and philosophical ponderings, and other defenses; (b) the ego and its functions, as seen in motor, perceptive, and thinking functions and total behavior; (c) status of emotional forces, as evaluated in degree and type of anxiety, emotions independent of external stimulus, degree of dominance of fantasy life, mood, level of organization of feelings, and (d) restitutional forces, as seen in religiosity, rebirth fantasies, and world reconstruction fantasies.

This, then, constitutes the conceptual tool of this study. It emphasizes a personality construct in which ego functioning, dynamically viewed, is the center of focus.

This tool resulted in a set of 120 psychiatric formulations for describing schizophrenia and their paired Rorschach indicators, each pair of which, for this research, may be regarded as a postulate. These psychiatric items were then used by the psychiatrists in describing each case, while the psychologists, working independently, made their description of each case from the Rorschach findings. By use of the Q technique (expertly described by William Stephenson in his chapter, "Q-Technique and the Rorschach Test"), these independent psychiatric and psychological appraisals were then correlated and factored for each patient. The factor analysis of the data resulted in a trait universe for six schizophrenic patterns which are summarized in Table 1 (Beck's Table 5, p. 132). The factors for the six schizophrenias, using the scheme of Table 1, are shown in Table 2 (Beck's Table 6, p. 132).

As is apparent, emphasis in this study was on a description of the manifestations and characteristics of schizophrenia, with light shed on this challenging psychological disorder from this point of view. One might wish, however, that these data had been *used* more by way of interpretation and integration for theory

TABLE 1
CONCEPTUAL SCHEME OF FACTORS IN SCHIZOPHRENIA

Theoretical Component	Factor	Level of Manifestation	
A. Defense Organization	I'	a. Withdrawal c. Pathogenic	b. Constriction
B. Intellectual Functioning	II'	a. Orderly	b. Disrupted
C. Fantasy Activity	III'	a. Autistic c. Little or none	b. Regressive
D. Social Adaptation	I''	a. Self-absorption c. Restitutional	b. Self-deprivation
E. Emotional State	IV'	a. Liability	b. Fixed Tone

TABLE 2
FACTORS FOR THE SIX SCHIZOPHRENIAS

Schizophrenia Types	Characterizing the Types				
S-1	A. c	B. b	C. c	D. a & c	
S-2	A. a & c	B. b	C. a & b	D. c	E. a
S-3	A. b	B. a	C. c	D. b	E. a & b
SR-1	A. b & c	B. b	C. c	D. b	E. a
SR-2	A. a & c	B. a	C. a & b	D. a	E. a
S-G	A. b	B. b	C. c	D. a	E. b

building concerning schizophrenic disorders, their origin, development, dynamics, and treatment. This wish is strengthened all the more because one is left with the desire for more integration of these data with other research on this problem. How earlier views of schizophrenia need to be modified as the result of this work is not sufficiently elaborated.

One wonders also what ideas these findings prompted in a Rorschach expert such as Beck concerning diagnostic tools for finer screening (detection and evaluation) of schizophrenic manifestations.

One might say that this study is more concerned, in a sense, with a comparison of the psychiatrists' and psychologists' formulations about schizophrenia than the value of these findings for a better understanding of schizophrenia. Such "profession"-orientation, while necessary as part of scientific "tool" evaluation, has its *raison d'être* in a continued problem-orientation.

This volume will interest not only psychopathologist and therapist, but all persons interested in efforts at a rapprochement of dynamic and trait approaches to personality theory.

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KENDALL, PATRICIA. *Conflict and mood*. Glencoe, Illinois: The Free Press, 1954. Pp. 182. \$3.50.

The studies reported in this small, lithoprinted volume are devoted to identifying factors leading to shift in responses to repeated attitude survey questionnaires and interviews. Analysis of panel studies conducted by the Bureau of Applied Social Research of Columbia University had shown that where first interviews were plotted against the second, marginal totals were generally quite constant, but sizable variation occurred in the cells. This instability of response, i.e., shifting from "agree" in the first interview to "disagree" in the second or vice versa, is called turnover. A special index of turnover was derived by Lazarsfeld, based on his latent structure model.

Conflict and mood were hypothesized as prime factors specifically related to response instability. Panel study results were analyzed and college student samples were used experimentally to determine if changes in responses upon repeated administration of questionnaires were associated with questions designed to produce conflict, and with self-ratings of mood states. By and large, the hypotheses were borne out. Additionally, panel studies were analyzed to identify other factors related to response instability: degree of respondent interest and concern with topic of question, question ambiguity, amount of forcing of opinion, and degree to which estimation of

facts was required of interviewer or respondent. These additional factors generally appeared to be related to instability in response.

From a methodological viewpoint, these studies are important in giving specification to factors that should be controlled in questionnaire construction. Respondent conflict and mood were the major variables studied. This reviewer however, would seriously question if one of the other factors was not indeed affecting the experimental responses analyzed in terms of conflict and mood. This other factor is the degree of respondent interest and concern. The author points out that this is a dimension of increasingly recognizable importance in surveys and that lack of interest leads to perfunctory, unstable responses. One gets the impression that this factor should have been taken into account in the college student experiment, and that instability of response may well have depended upon it to a sizable extent.

The fourfold table is typical of the sociologist's approach to data analysis. Correlation analysis, linear scaling, and the like would undoubtedly make this book more to the liking of psychologists in that a more refined degree of estimation of the influence of the variables would be possible. But this in no way invalidates the findings at the level of analysis used. One final point: in the reviewer's opinion, this report would have been better produced as a monograph with an appropriate sociological research title instead of a flashy, clinical-sounding one.

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PIÉRON, H., REUCHLIN, M., BIZE, R.,
BÉNASSY-CHAUFFARD, C., PACAUD,
S., & RENNES, P. *Traité de psy-*

chologie appliquée. Livre Troisième. L'Utilisation des aptitudes, orientation et sélection professionnelles. Paris: Presses Universitaires de France, 1954. Pp. lx+342-757.

This 415-page book is volume three in a projected seven-volume work on applied psychology being brought out under the editorship of H. Piéron. Four of the six co-authors contribute a chapter each, a fifth, two and the sixth, three chapters to the book. The nine chapters are divided into two parts, five of the chapters being subsumed under the general heading of professional orientation, and the remaining four under that of professional selection.

In the opening chapter of the work, the editor of the series calls attention to the importance of becoming professional regarding the utilization of aptitudes and the scope of his appeal is nationwide. The other four chapters in the first part deal successively with surveys regarding current knowledge of different kinds of aptitude, the criteria in terms of which prediction regarding vocational success can be made, the varying demands of different trades and occupations on human resources, and different modes and degrees of adaptation made by the individual upon entering a profession. In the last chapter of this part are also considered the special problems of fatigue, monotony, accidents, and handicaps. The longest chapter in the book, covering one hundred pages or almost one fourth of the entire work, is that concerned with the specific demands of different kinds of tasks and occupations.

The four chapters of part two are subsumed under the general heading of professional selection. Treatment in this part of the work is perhaps more specific than that in some chap-

ters in the first part. In the first chapter here different aspects of the comparatively new province of human engineering are treated. Ways in which psychological and psychophysiological studies in a variety of tasks have led to greater economy and efficiency of effort are indicated. The various factors which have been found to contribute to error are evaluated in the second chapter. The important concept of validity is considered in relation to the use of batteries of tests in the third chapter. The final chapter in the book deals with the problem of classification in government employment, in industry, and in other spheres of action.

To each chapter is appended a quite extended bibliography, except that one bibliography is given for the three chapters contributed by the same author. Most of the references are to comparatively recent studies, very few going back beyond the time of World War I. Among the few whose works are mentioned as far back as the opening of the twentieth century and before are F. Galton, J. McK. Cattell, A. Binet, J. Jastrow, and T. L. Bolton. Excepting one list in which all but one of the references are in French, the lists include in several cases at least a majority of studies in English.

It is not yet possible to evaluate this book in relation to the rest of the series of which it is a part. In itself, the work deals with aspects of general personnel problems which have been receiving increasing attention in the past two or three decades. The co-operation of several specialists seems well justified, since it would not easily be possible for a single writer to survey the literature in the many aspects of this complex problem which has manifested such rapid development in a very few years. In spite of some differences in literary

style, the treatment by different writers is uniformly high and the plan of organization is well conceived. The work perhaps would be more easily used if a general index had been added.

This work represents a survey of the variety of ways in which psychological methods—testing, factor analysis, job analysis, etc.—have been used in connection with different aspects of the complex personnel problem. Activity on this psychological front recently has been rapid. It is to be expected that in the future existing methods will continue to be adapted to the needs of still other practical problems, and that new methods will be developed to meet the needs of analytic studies regarding aspects of the problem to which the old ones do not apply. As a starting point for further efforts in this important field, this work is bound to serve a very important purpose.

MICHAEL J. ZIGLER

Wellesley College

BACH, GEORGE H. *Intensive group psychotherapy*. New York: Ronald, 1954. Pp. xi+446. \$6.00.

Bach has had a lot of experience with group psychotherapy, and he writes about groups with a fine combination of scholarship and clinical sense. His book is an exciting one, for a number of reasons. First it presents a new concept of how to work with groups for therapeutic purposes. Then it richly embroiders on its central thesis through a large number of descriptions of clinical problems and their solution in the group. There is an astuteness in conception and a dispatch in execution of clinical tactics that a less bold group therapist observes with respectful admiration. Then there is wide-ranging scholarship, with citations from many fields of psychological literature, illuminat-

ing myriad aspects of the group therapy process. But finally, and I regret to have to write this, the book has annoying qualities which detract from its effectiveness. It should be a much better book than it is. Mainly it is lacking in discipline. Theory and clinical practice are not integrated; the precise relevance for intensive group psychotherapy of citations from the literature is often illusive; imprecisions, overgeneralizations, and inconsistencies are all too frequent. But let's be more specific, mostly about the strengths of the book, for these considerably outweigh its weaknesses. The weaknesses one may regard as minor annoyances that can be lived with more or less comfortably while finding out how Bach does psychotherapy with groups. There's a lot to learn about that.

The main idea of intensive group therapy is to heighten the therapeutic significance of the group by requiring that the conflicts of the individual be worked out in his immediate and contemporaneous relationships with other members of his group. The group experience is not an auxiliary to other therapies; on the contrary, all therapeutic endeavors are made to coalesce in the group. That group interactions provide opportunities for clarification of a person's difficulties and for practice of new modes of relating to others is an idea common to many approaches to group therapy. What is new and significant here, as the name of the method implies, is the *intensity* of the relationships that Bach's methods generate. How does he intensify the relationships? A careful reading of the first section of the book will provide abundant illustrations, only a few of which can be given here. For one thing, the groups meet frequently, once or twice a week for two hour periods, and for a large

number of sessions (four of nine patients described had met more than 170 times). The two-hour office sessions are followed by a required two-hour postsession at dinner without the therapist being present. Postsession occurrences become appropriate material for analysis in the next formal hour. When individual sessions are held with group members, the focus is on the group experience, and the expectation is that the events of the individual sessions will be shared with the group. New participants are encouraged to record their reactions to members of the group and then read their notes to the group. Dreams, drawing, role-playing, and therapist-planned activities are used to actualize the patient's problems in the group. Bach is very inventive in finding ways of bringing a person's problems into focus in the context of the group. There is an accent on experience, on practice in relationship to people present, on deriving meaning from the immediate experience in the group: "... the patient is literally 'caught in the act' by his own behavior." Especially helpful in the clinical section of the book is the inclusion of verbatim protocols and reproductions of drawings produced in the groups.

Bach's collections of "do's and don't's" for group therapists is a good example of how profitably he has reflected on his experiences; these admonitions could be used as a framework for an entire course on group psychotherapy. What of outcomes? Bach is not alone among group therapists in being able to say little about this. While there is no quantitative evaluation of the effectiveness of the method, there is a consistent clinical viewpoint which has been sufficiently well defined to be tested in the future.

The book aspires to integrate clinical practice with Lewin's field con-

cepts, but the effort does not quite come off. Though Part III of the book turns theoretical and elaborates on the implications of group dynamics for group psychotherapy, the methods recommended by Bach seem less required by the theory than one is led to anticipate. They are not in conflict; they just don't mesh in any convincing way. Bach is obviously a very creative clinician; I surmise that he also has a scholarly conscience that makes theory attractive even though not compellingly relevant. In Part II, however, are many sound and provocative observations about groups. They hardly constitute an integrated theory, but they are valuable observations—a step between clinical practice and a theory.

Now for the annoyances. Bach "writes loose," and it is often hard to know just what he believes. What is his position with respect to tests, for instance? He "always" gives an elaborate battery of tests to prospective group members and then dismisses test results as "neither specific enough nor stable enough for the prediction of actual interpersonal behavior in groups." With unabashed confidence, which seems inversely proportional to the amount of solid evidence on the subject, he dismisses the family as a psychologically significant source for experience for modern man and substitutes the peer group. He rejects, soundly I think, the family as a model for the group therapy situation. But one of his arguments—that there are too many different kinds of family patterns to use "the family" as a model—he ignores when he recommends the peer group as a model. Surely there are hundreds of different patterns of peer group relationships. Finally, to mention only one other item, Bach seems occasionally to jettison the essential concept of prob-

ability in behavior and describes events as occurring "always," "never," "inevitably," and "invariably." As I said, it's loose. But such distractions are minor if one keeps turning to pages where Bach describes his clinical procedures and his immediate rationale for them.

NICHOLAS HOBBS

Peabody College

LEWIS, HILDA. *Deprived children*. New York: Oxford Univer. Press. 1954. Pp. xviii+163. 9s. 6d. net.

This compact and carefully written book is a report of a social-clinical study of 500 children entering the Mersham Reception Centre in Kent, England, between October, 1947 and July, 1950. Three concerns are evident: (a) an evaluation of the work of the Centre; (b) an evaluation of the effects of earlier maternal separation on the child's adjustment at the time of admission and on his adjustment two years later; and (c) an evaluation of the factors leading to the child's placement at the Centre and the importance of these factors for his subsequent adjustment.

The writer has with care considered the family backgrounds, socioeconomic influences, and personal experiences of these children prior to their admission, and she has related these variables to the personality and behavior patterns of the children. The large number of variables which she considers in these areas and the skill with which she has interrelated these factors, sets this research report in marked contrast to the speculation characterizing most of the reports on deprived children. While this is no small achievement, perhaps her main contribution has been in showing that scientific methodology and statistical techniques may be fruitfully used in investigations in this area.

A number of limitations on the

data on which the report was based should be noted, although perhaps most of them were acknowledged by the writer; the diagnosis, ratings, and evaluations were apparently carried on by only one individual, the writer—hence, their reliabilities are indeterminable. There was no control group and there is question as to whether the compared groups were always comparable on variables other than on the one under consideration. The criteria employed were not always specified, although it must be granted that they were given more precisely and frequently than in any other study in the area to date. While there is little doubt that the foster homes were superior to the "true" homes in many respects, in what respects it is impossible to determine from the book. Such information would have added materially to our knowledge of what kind of children should be placed in what kind of homes, and to the value of an already excellent book.

The author found in her investigation that the degree of disturbance of the child entering the Centre is related to his mother's emotional instability, her intellectual level, her tendency for overindulgence, her tendency to neglect and/or to reject him, etc. However, she found that there was not a significant relationship to the presence or lack of availability of the mother. She found that "parental rejection" was significantly related to the child's manifestation of "unsocialized aggressive behavior," that "neglect and bad company" were significantly related to "socialized delinquency," and that a background of "constraint" was significantly related to "neurotic behavior." In her two-year follow-up she found that the condition of those placed as recommended was superior to those not placed as recommended and that

there had been considerably greater improvement in the former group. The preceding results are only a sample of the findings she reports.

The author is to be commended for her research contribution, and for her very readable report. The book should be read by all concerned with the welfare of the child.

SAMUEL R. PINNEAU

University of California

ARNHEIM, RUDOLF. *Art and visual perception*. Berkeley and Los Angeles: Univer. of California Press, 1954. Pp. x+408. \$10.00.

Although art is one of the most fruitful areas of application of scientific psychology, it has received relatively little attention from psychologists. One can count the books dealing with art by psychologists on the fingers of one hand, and psychological studies dealing with art are few and far between. In reading this book, one realizes why more psychologists have not been concerned with art. Art is a technical specialty in its own right and one must be expert both in psychology and in either creative art or the history of art to write on art. Arnheim's book brings the scientific knowledge of a trained psychologist to bear on the fundamental problems of visual art as it has developed through the ages. The discussion is always with reference to concrete works of art. Paintings, designs, figures, and sculptures are chosen to present the problems of balance, shape, form, growth, space, light, color, tension, and expression operationally, almost experimentally. Many original drawings, diagrams, and figures illustrate basic principles and important points. The writing is superb. The book is full of penetrating insights into questions of art and also into many problems of concern to the psychologist.

Fundamentally this book is an argument against the usual art historian's approach, so well described by Arnheim as the purely subjective point of view, that what a person sees in a work of art (and, we may add, what the artist creates) "depends entirely on who he is, what he is interested in, what he has experienced in the past, and how he chooses to direct his attention" (p. 64). Against this view, Arnheim marshals visual phenomena independent of individual experience: phenomena demonstrating simplicity, subdivision of wholes, grouping, formed patterns, and the other many properties well known to psychologists through the work of the Gestalt group. Three basic principles, of which one is methodological and two are explanatory, dominate the author's argument. The methodological principle assumes that immediate perception furnishes the clues to the work of art, since it reveals the figural properties determining form orientation, tridimensionality, subdivision, and grouping of complex patterns. If "these patterns have an objective structure of their own, they are likely to offer a solid basis on which the artist can rely" (p. 64). The two explanatory principles are simplicity of structure and dynamics of brain processes. The importance of these principles can perhaps best be appreciated by noting that they serve in place of learning, culture, historical accident, and individual genius to account for why things look as they do and why artists draw and paint as they do.

The degree to which the reader will accept Arnheim's principal assumptions is probably a personal matter. This reviewer goes along with the evidence of phenomenology, immediate experience, up to a certain point, and then finds he cannot go along the rest of the way. Thus, he is willing to

accept phenomenological evidence that what we accept as good representations of reality is a matter of familiarity and adaptation, and changes with the times. We are unaware of the distortions of our own time and culture. But he cannot accept the author's explanation of children's drawings and paintings as the result of the fact that they reproduce what *they* see. This account is not convincing to an adult such as the reviewer, whose drawing ability never went beyond that of the lower quartile of six-year-olds. In view of the inchoate character of much modern art, it may be naive to assert that the artist must learn how to express what he sees and what he wants to convey just as the child must learn a language to express what he wants to say. And even though it may be granted that modern art requires a technique as much as representational art, nevertheless, *it* must be learned, although it is a different technique from classical representational art.

Similarly, one may follow the author up to a certain point in his reliance on the principle of simplicity to account for the way things appear as they do. For the fairly simple figures considered by Arnheim we may agree with him that "... when three-dimensional perception occurs (with certain figures) we must assume that undistorted shape in a tilted position makes for a simpler total situation than distorted shape in frontal position" (p. 205). But we see many distorted shapes in the frontal-parallel plane which do not go three-dimensional to be seen more simply; and as I write, my desk beside me is a wurr-wurr of regular shapes in random orientations giving an impression of disorder which no simplifying tendency of perception, no balancing of brain

forces, makes orderly. And so with much in art, particularly modern art.

Arnheim recognizes that simplicity may be overworked for "... if the tendency to simplicity persisted all the way, there would be no seeing whatsoever" (p. 359). In his admission that the reality-level of art changes with familiarity he recognizes that learning and cultural factors also operate to determine what is perceived. That the traditional use of learning and familiarity factors in explaining perception was inadequate few would now deny, but this fact should not prevent us from incorporating their true features into more adequate theories. At this point, the reviewer makes bold to assert, adaptation-level theory could have been of service in providing a rational basis for the operation of learning factors in perception.

The discussion of color leaves one who has devoted considerable attention to this subject somewhat confused. Present-day psychologists do not speak of green as yellowness plus blueness, even though they know that blue and yellow pigments yield green when mixed. Nor is orange the complement of blue. An arrangement of the hues in a color circle that does not include green as a "fundamental" color (primary, unitary?) will also puzzle psychologists. In denying that complementary colors can be specified in terms of secondary standards (pigmented papers, for example) Arnheim overlooks the fact that both the Munsell and Ostwald color systems specify complementaries as well as color matches. Nor does Arnheim make use of the best data on color preferences in his theory of color harmony. While the experimental work on color harmony is not fully adequate for explaining all the uses of color by artists, psychologists would be happy if Arnheim had made more

use of such experimental results as are to be found in the literature (Guilford's, for instance) in presenting his own theory of color harmony.

These criticisms are offered in a constructive spirit, for no one reading this magnificent book can fail to appreciate its splendid contributions. It has illumined many purely psychological problems for the reviewer as well as widened and deepened his appreciation of art. And a book which reflects so well the author's urbanity, catholicity, and keenness of mind, as well as his technical grasp of the scientific and the artistic, is no small achievement. It should be in every psychologist's library and a required reference in courses dealing with vision and perception.

HARRY HELSON

University of Texas

MEEHL, PAUL E. *Clinical versus statistical prediction*. Minneapolis: Univer. of Minnesota Press, 1954. Pp. x+149. \$3.00.

Meehl attempts to plot a course to the port of valid prediction through the "rigorous" channels of statistical methods and the "sophisticated" undercurrents of clinical dynamics. If these adjectives seem prejudicial, one may select others from four lists of some 20 words each (p. 4), all of which are very handy for stereotyping either the statistical or clinical methods of prediction. Terms like operational, communicable or verifiable may be used when statistical methods are favored while mechanical, atomistic, or additive are available if such methods are disliked. Similarly, proponents of clinical methods may favor holistic, patterned, or organized, while opponents may prefer mystical, subjective, or intuitive. As the author points out in the preface, students have reacted to his lectures on this topic as to a

projective technique and it is expected that readers will do likewise according to their biases.

Most of the book is devoted to a thoughtful but discursive analysis of the alternative strategies for prediction of statistical or actuarial methods as compared with clinical or case-study methods. Meehl believes that much confusion could be avoided if careful distinction were made between two different uses of statistics, the types of data involved in prediction, and the methods of combining data in making predictions. One use of statistics is referred to as discriminative or validating while the other is called structural or analytic. Although this differentiation is debatable, there can be little quarrel with the conclusion that predictions, however arrived at, must ultimately be evaluated by statistical procedures—"always... the shadow of the statistician hovers in the background" (p. 138). In contrast Meehl thinks there is little likelihood that clinical hypotheses can ever be generated by formal rules of logic and statistical operations.

The paucity of empirical evidence regarding the relative "batting averages" of clinical vs. actuarial prediction is emphasized in Meehl's summary of some 20 possibly pertinent studies. By some rather heroic analysis, Meehl concludes that "in all but one... the predictions made actuarially were either approximately equal or superior to those made by a clinician" (p. 119). Unhappily, it is questionable how closely any of the studies approaches an ideal comparative design. There appears to be a clear need for more exacting and systematic comparison of the two methods of prediction with respect to both predictive efficiency and relative costs. Meehl does an excellent job of raising some of the basic prob-

lems and issues which have to be considered in carrying out research in this area.

LEONARD S. KOGAN

*Institute of Welfare Research
Community Service Society of New York*

ROTTER, JULIAN B. *Social learning and clinical psychology*. New York: Prentice-Hall, 1954. Pp. xvi+466. \$6.50.

In the preface Rotter says: "The purpose of this book is to arrive at a systematic theory from which may be drawn specific principles for actual clinical practice, and to illustrate some of the more important applications of the theory to the practice. Rather than attempt to apply this theory to all the problems facing the clinical psychologists, we have chosen to apply it to only two of the clinician's most important problems—the measurement of personality (personality diagnosis) and psychotherapy. Even in these broad areas the application is more illustrative than comprehensive."

The first three chapters ("The Importance of Theory in Clinical Psychology," "Some Major Problems of Clinical Psychology," "Criteria for a Language of a Description for Clinical Psychology") represent for the most part a clear and incisive introduction to the major purpose of the book, chapters which can be read with profit by all clinical psychologists. The next four chapters, which represent the bulk of the book, contain the aims and concepts of Rotter's social learning position as well as the ways in which it differs from other approaches. It is impossible in the scope of this review to describe and evaluate the basic concepts of Rotter's position. There is, however, one concept which deserves, in this reviewer's opinion,

special emphasis, namely, expectancy: the "... probability or contingency held by the subject that any specific reinforcement or group of reinforcements will occur in any given situation or situations." Although, as Rotter points out, this is not a new concept, he gives it an importance which it has heretofore not had in systematic theory. It would perhaps be more correct to say that he states this concept in a way which has great relevance for clinical practice, although he does not pursue this relevance as much as this reviewer would have liked. Few, if any, would quarrel with Rotter when he says: "The understanding and prediction of changes in expectancies is one of the more crucial aspects of this theoretical approach. Not only is it true from our formulations that behavior depends, in part at least on expectations, but reinforcement values themselves depend upon expectations for subsequent reinforcements. Consequently, understanding how expectations change becomes of crucial significance for any application of this theory to psychotherapy or any effecting of behavioral change." Unfortunately, in the chapter on psychotherapy one does not find examples or descriptions of how expectancies are deduced by the therapist, how they change as therapy progresses, and how awareness of the importance of expectancies determines therapeutic tactics and outcomes. To do justice to what Rotter considers to be crucial problems it would have been necessary to present and discuss the data of psychotherapy. It is more than surprising that not a single therapeutic interaction, or any part of one, is presented. In terms of the stated purposes of this book, this lack is indeed serious. In fact, in the entire book there is not anything (with one possible excep-

tion) resembling a clinical case—a lack which makes one wonder why "clinical psychology" is in the title of the book. Is it carping or hypercritical to expect that an attempt to apply a systematic theory to clinical problems would involve presentation and discussion of the data with which the clinician deals? Can you talk very meaningfully about psychotherapy without therapeutic data?

The above criticisms, stated in terms of the concept of expectancy, hold for most of Rotter's other formulations. To make the objection clear: this reviewer feels that Rotter has not come to grips with those clinical problems which his theoretical formulations were intended to illuminate. If he had given as much systematic thought to the clinical problems as he did to the statement and buttressing of his theoretical formulations, this book would have been more of a significant contribution than it is.

There is one other major criticism which has to be made. Rotter's discussion and evaluation of psychoanalytic theory is amazingly superficial and, to the unwary graduate student, misleading. For example: "Questions such as why children around the age of six show considerable attachment to parents of the opposite sex, why people become aggressive following frustration, why people show attraction to the same sex, and so on, are answered [by psychoanalysts] 'They have inherited an energy that directs them to do it.' Freud did state that the particular form which the expression of the energy took was perhaps a function of learning or experience and that behavior itself, over a long period of time or in adulthood, was determined by what happened to the person when these instinctual energies were expressed in early childhood. To the

ultimate 'whys' Freud could say little other than because it is an instinct." This reviewer submits that this quotation, which is fairly representative of Rotter's handling of Freud, reflects a distorted and biased understanding of Freud's position. But why does Rotter focus only on Freud?; he explicitly points out that there have been changes in psychoanalytic theory. An author has to select what he will talk about—but is age of the concept an adequate criterion for selection? Should an evaluation of learning be concerned only with Pavlov?

Some very positive things should be said about this book. First, it represents one of the few attempts to formulate and apply a learning theory to clinical phenomena and problems—the more such courageous attempts we have, the better will we be able to evaluate the adequacy of such theories. Second, Rotter's formulations have generated a relatively large number of studies at The Ohio State University—a tribute not only to Rotter's effectiveness as a teacher but a reflection of the fruitfulness of the formulations. As Rotter and his students continue to apply these formulations to complex clinical phenomena, we will be in a better position to evaluate the degree of fruitfulness of their theoretical approach.

SEYMOUR B. SARASON

Yale University

HYMAN, H. H., et al. *Interviewing in social research*. Chicago: Univer. of Chicago Press, 1954. Pp. xvi + 415. \$8.00.

This volume is a scholarly but free-flowing treatise on sources of error and potentialities for their control in the *survey interview*. It is the report of a six-year collaborative undertaking by the National Opinion Research Center (NORC), commissioned by

the Joint Committee of the Social Science Research Council and the National Research Council on the Measurement of Opinion, Attitudes, and Consumer Wants, and sponsored by the Rockefeller Foundation. A workmanlike job has been done of rendering a pattern of reasonable speculations from a conflictful mass of evidence and pseudo evidence. Although results would have appeared more consistent had the authors confined themselves to studies carried out under their own auspices, they elected to attempt the more difficult but valuable task of integrating their own findings with the available literature, studies by other agencies, and the opinions of experienced workers in the field. The end product is an excellent and up-to-date summary of an obviously complex subject.

The book is divided into seven rather lengthy chapters. Chapter I, entitled "A Frame of Reference for the Study of Interviewer Effects," reviews the widespread application of interviewing as a fallible method of scientific inquiry, and cautiously suggests the possibilities for generalization about other kinds of interviewing from studies of the survey interview. Interview error is noted as being of two major types—interviewer variation or reliability and accuracy or validity. Somewhat surprisingly, the authors appear to entertain the notion that maximum reliability is somehow incompatible with maximum validity, each being a function of differential locus along a continuum of freedom allowed the interviewer.

Chapter II, "The Definition of the Interview Situation," is devoted to the formulation of hypotheses about factors affecting interviewing from "phenomenological" case studies based largely on intensive interviews with interviewers and respondents.

The procedures used in these case studies are elaborated in Appendix A. Some will note with satisfaction the special role of the nonsurvey interview in developing a framework for inquiry about the survey interview.

The third chapter presents an analysis of "Sources of Effect Deriving from the Interviewer." The major conclusion drawn from a series of studies is that the interviewer's own opinions (ideological bias) generally affect the results of survey interviews to a less important degree than do his expectations based on the respondent's early answers (attitude-structure expectation), judgment of the respondent's group membership (role expectation) or the interviewer's beliefs regarding prevailing opinion in the population (probability expectation).

Chapter IV, "Respondent Reaction in the Interview Situation," reviews the contradictory evidence on personal interviewing vs. self-administered questionnaires as well as the systematic and differential effects of disparities between interviewer and respondent with regard to sex, age, group membership, class, and so on. The response of the respondent in the interview is seen to be a function of his task involvement and his social involvement.

The "Situational Determinants of Interviewer Effects" are examined in Chapter V. Among such determinants which influence the psychological processes of the interviewer and respondent, and their interaction in the interview, are respondent anonymity, survey sponsorship, structuring of the interview procedure, form and content of the survey, and method of recording.

Chapter VI is an attempt to evaluate the magnitude of "Interviewer Effects under Normal Operating Conditions." On the basis of an admittedly small body of evidence, the conclusion is reached that "for most fixed-response opinion questions there is relatively little inter-interviewer variation." Although this conclusion seems somewhat optimistic in the light of the evidence, it seems clear that interviewer variation tends to be relatively smaller for fixed-response opinion questions and factual questions than for free-response questions and field ratings by interviewers.

The final chapter is devoted to consideration of the relative merits of various methods for "Reduction and Control of Error." The first portion of the chapter implements the findings presented in preceding chapters by describing possibilities for control of error arising from the interviewer through better selection and training, control of error arising from respondent reactions by matching characteristics of interviewer and respondent, and control of situational errors by standardization of presumably optimal interview procedures. The final section treats of the indirect control of error by statistical estimation of its magnitude and components.

In recognition that many of the findings reported are based on and may be limited to the kinds of interviewers hired by NORC, Appendix B describes NORC training and field procedures, and the characteristics of the NORC national field staff.

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*Institute of Welfare Research
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New York*

ERRATUM

In the review of Roger G. Barker of *Manual of Child Psychology*, edited by Leonard Carmichael, which appeared in the May 1955 *Psychological Bulletin*, there is a typographical error. The sentence on page 266, right-hand column, should read "However, this reader found intriguing the world view so *boldly* sketched."

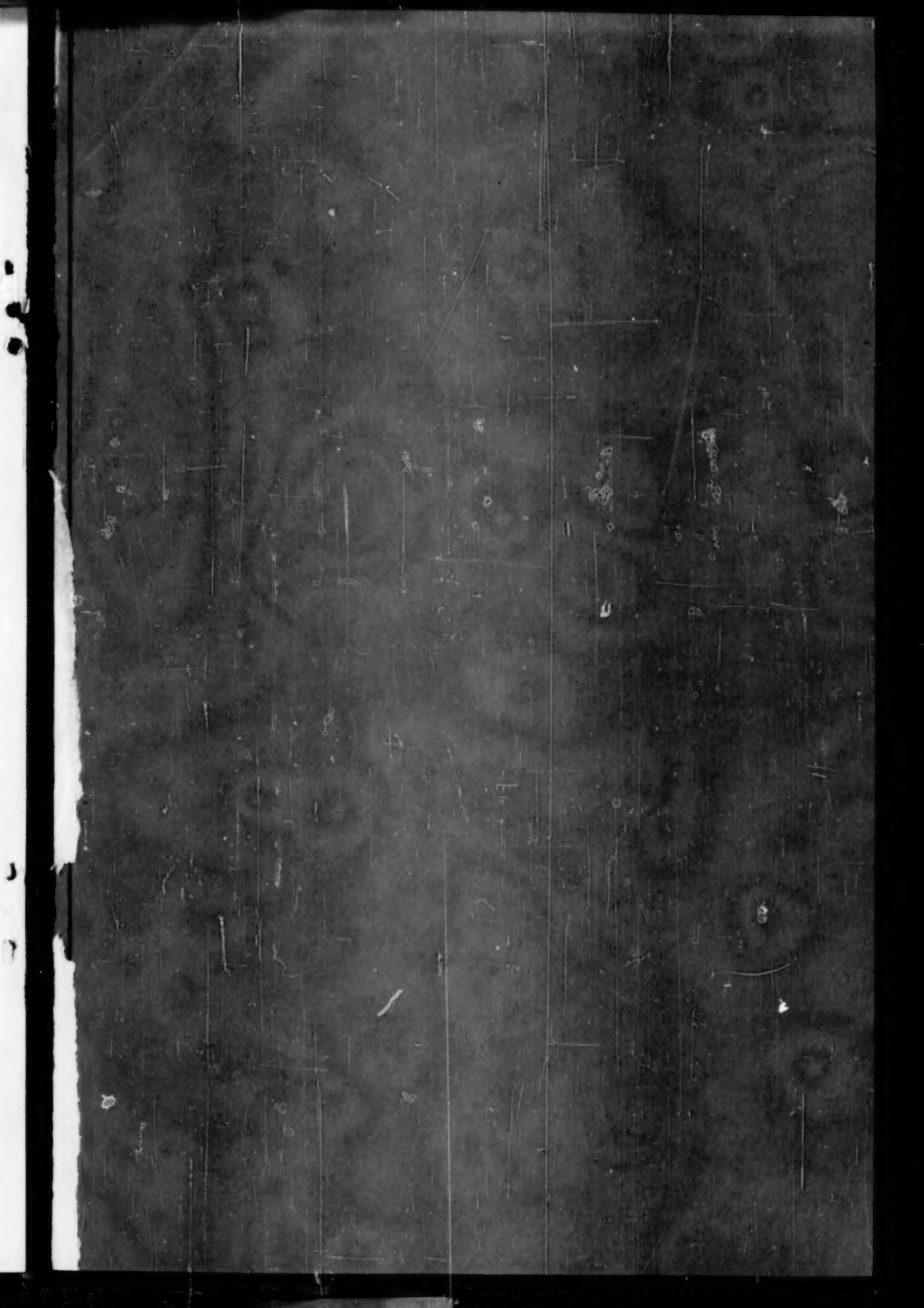
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IMPORTANT ANNOUNCEMENT

As approved by the Board of Directors and the Council of the APA, beginning with January 1956, a new journal will be published by the Association. This journal will review books, monographs, films, and related publications—a function presently performed by four different APA journals.

Accordingly, book reviews will appear in the *Psychological Bulletin* only through the completion of the present volume, 52, November 1955 issue.

Henceforth all publications submitted for review and requests to prepare reviews should be directed to the editor of the new journal:

Contemporary Psychology, A Journal of Reviews

E. G. Boring, Editor
Memorial Hall
Harvard University
Cambridge 38, Massachusetts

The journal will appear monthly and the subscription price (for non-APA members) will be \$8.00. Subscription orders should be sent to:

American Psychological Association
1333 Sixteenth Street N.W.
Washington 6, D.C.